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THE GROWTH OF OUR KNOWLEDGE OF HELIUM.

THE DISCOVERY OF THE LINE D^3, 1868.

IN the year 1868, spectrum analysis was first utilised in endeavouring to unravel the message which was conveyed to us by a most interesting eclipse observed in India. The diagrams will indicate the kind of record with which we have to deal in studying these celestial hieroglyphics. We are in one part dealing with the long waves of light, the red; we are in the other dealing with the shorter waves of light, the blue. The work done in that eclipse is indicated by the bright lines—the hieroglyphics—which, when translated as they have been, describe for us the chemical nature of the particular stuff in the sun, which made him put on a blood-red appearance "on his getting out of his eclipse". Taking the notes in the light scale which are lettered in the ordinary spectrum of sunlight, in order that they may be easily recognised and remembered, we learn the particular qualities of the light emitted by the blood-red streak.

We have one quality represented by the line D, another at C, and another at F. According to the diagram, one of the lines is in the position of D. One observer said it was "at D, or near D".

Soon after this eclipse was observed in India, a method,

long before formulated, of studying the blood-red streak surrounding the sun without waiting for an eclipse was brought into operation.

By this method it was quite easy to make observations whenever the sun was shining, perfectly free from any of the difficulties attending the hurry and the worry and the excitement of an eclipse, which lasts only a few seconds.

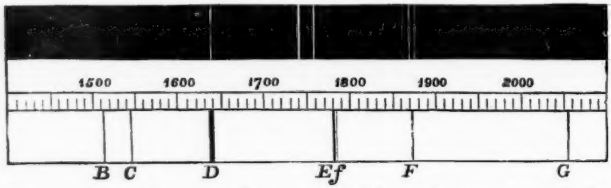


FIG. 1.—Pogson's diagram of the spectra of the sun's surroundings in the Eclipse of 1868. The bright lines seen are shown in the upper part of the diagram; the chief lines in the solar spectrum, red to the left, blue to the right, are shown in the lower part.

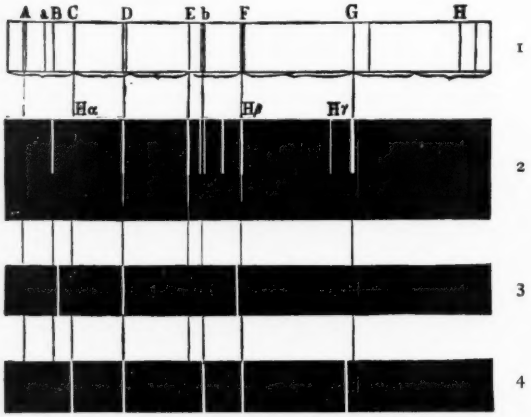


FIG. 2.—Summation of the observations of the spectrum of the sun's surroundings in the Eclipse of 1868. (1) Solar spectrum showing the position of the chief lines. (2) Rayet's observations of bright lines. (3) Herschel's observations of bright lines. (4) Tennant's.

Further, as the method consists of throwing an image of the sun, formed by a telescope, on to the slit of a spectroscope, so that the spectrum of the sun's edge and of the sun's surroundings can be seen at the same time, exact coincidence or want of coincidence between the bright and dark lines can be at once determined. During an eclipse

this of course is not possible, as the ordinary spectrum of the sun, with its tell-tale dark lines, is invisible because the sun, as we ordinarily see it, is hidden by the moon.

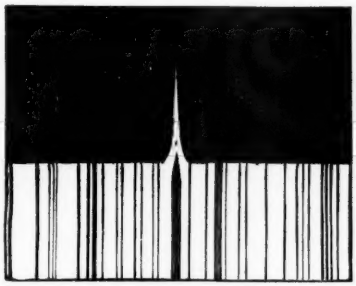
Working, then, under such very favourable conditions, it was seen that there was certainly a red line given by this lower

C

FIG. 3.—The exact coincidence of the red line with the dark line C.

part of the solar atmosphere coincident with the very important line in the solar spectrum which we call C.

Another part of the spectrum in the blue-green was examined, and there again it was seen that the parts outside the sun gave us a bright line exactly in the position of



F

FIG. 4.—The exact coincidence of the blue-green line with the dark line F.

the obvious dark line in the solar spectrum which is called F; so that with regard to those two most important lines, there was no doubt whatever that we were dealing with the substance which produces these dark lines in the solar spectrum.

Fig. 5 is a diagram of the yellow, or rather the orange, part of the solar spectrum, showing two very important lines, which are called the lines D, due to the metal sodium, the investigation of which was just as important in solving the celestial hieroglyphics we call spectral lines as the Rosetta stone was important in settling the question of the Egyptian ones.

Pogson, in referring to the eclipse of 1868, said that the orange line was "at D, or near D". We see from the

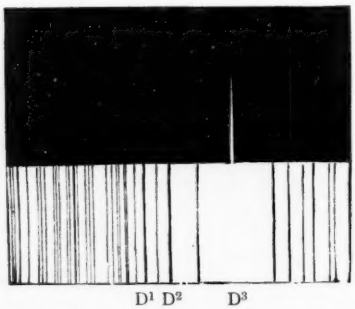


FIG. 5.—The want of coincidence of the orange line D^3 with the dark lines D^1 and D^2.

diagram (Fig. 5) that the new method indicated that "near D" was the true definition. The line in this position in the spectrum, unlike the other two lines which I have indicated, has no connection at all with any of the dark lines in the ordinary solar spectrum. We were therefore perfectly justified in attaching considerable importance to this divergence in the behaviour of this line, taking the normal behaviour to be represented by the two strong lines in the red and the blue-green. The new line was called D^3 to distinguish it from the sodium lines D^1 and D^2.

A considerable amount of work was done with regard to the orange line. It was found that there was no substance in our laboratories which could produce it for us, whereas in the case of the line D we simply had to burn some sodium, or even common salt, in a flame to produce it, and the other lines in the red and the blue-green were easily made manifest by just enclosing hydrogen in a vacuum tube, and passing an electric current through it,

or observing the spectrum of a spark in a stream of coal-gas.

Now at the first blush it looked very much as if this line was really due to the same element which produced the others at C and F, and it was imagined that the reason we did not see it in our laboratories was because it was a line which required a very considerable thickness of hydrogen to render it visible. That was the first idea, and Dr. Frankland and myself found that there was very considerable justification for this view, because a simple calculation showed that the thickness of the solar atmosphere, which was producing that orange line under the conditions which enabled us to see it in our instruments by looking along the edge of the sun, was something like 200,000 miles.

A B C

FIG. 6.—Changes of wave-length of the F hydrogen line when a solar cyclone is observed. A, the change towards the red indicates the retreating side of cyclone. C, the change towards the blue indicates the advancing side. B, the whole cyclone is included in the width of the slit, and both changes of wave-length are visible.

Hence, in order to get a final decision on this point, there was nothing for it but to tackle the question from a perfectly different point of view, and the different point of view was this. The work had not gone on very long before one found minute alterations in the positions of these lines in the spectrum; the orange line, for instance, might sometimes be slightly on one side, and sometimes on the other of its normal position. Further work showed that in these so-called "changes of wave-length" we had a precious means of determining the rate of movement of the gases and vapours in the solar atmosphere.

Fig. 6 indicates how these changes of wave-lengths are

shown in the spectroscope. The lines are contorted in both directions, and sometimes to a very considerable extent, indicating wind movements on the sun, reaching and sometimes exceeding 100 miles *a second.*

We had here a means of determining whether the orange line was produced by the same gases which gave the red and blue lines, because if so, when we got any alteration in the position of the red and blue lines, which always worked together, we should get an equivalent alteration in the position of the orange one.

I found that the orange line behaved quite differently from either the red or the blue lines; so then we knew that we were not dealing with hydrogen; hence we had to do with an element which we could not get in our laboratories, and therefore I took upon myself the responsibility of coining the word helium, in the first instance for laboratory use.

This kind of work went on for a considerable time, and what one found was, that very often in solar disturbances we certainly were dealing with some of the lines of substances with which we are familiar on this earth; but at the same time it was very remarkable that when the records came to be examined, as they ultimately were with infinite care and skill, it was found that not only did we get this line in the orange indicating an unknown element associated with substances very well known, like magnesium, but that there were many other unknown lines as well. Within a few months of my first observations, several new lines about which nothing was known were thus observed.

THE DISCOVERY OF OTHER UNKNOWN LINES, 1869.

The place of the orange line D^3 I determined on 20th October, 1868. Among many other lines behaving like it, two at wave-lengths 4923 and 5017 were discovered in June, 1869, and afterwards another at 6677, while Professor Young noted another in September, 1869, at 4471. He wrote:—

"I desire to call special attention to 2581·5 [=4471 on

Kirchhoff's scale], the only one of my list, by the way, which is not given on Mr. Lockyer's. This line, which was conspicuous at the Eclipse of 1869, seems to be *always present* in the spectrum of the chromosphere. . . . It has no corresponding dark line in the ordinary solar spectrum, and not improbably may be due to the same substance that produces D^3."

This same line was noted also by Lorenzoni and named *f*. Another line at 4026 was added later by Professor Young.

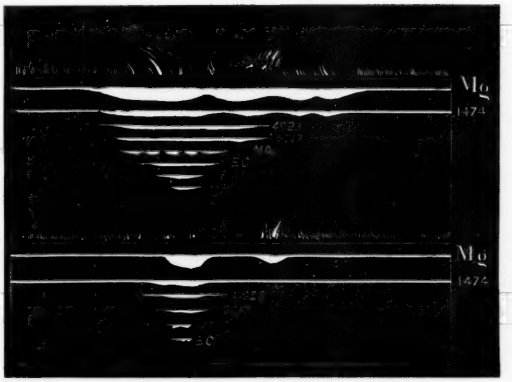


FIG. 7.—Tacchini's observations of two slight solar disturbances showing the height to which the layers of the different gases extend. Magnesium vapour is highest of all, and is furthest extended; next comes a gas of still unknown origin, indicated by a line at 1474 of Kirchhoff's scale and so on.

Then with regard to solar disturbances. Let me refer in detail to a diagram indicating some results arrived at by the Italian observers. We are dealing with the spectroscopic record of two slight disturbances in a particular part of the sun's atmosphere. The spectroscope tells us that in that region there was a quantity of the vapour of magnesium which is collected in that place. Then we find that another substance, about which we again know nothing whatever, is also visible in that region, and then we get the further fact that in those particular disturbances we get four other spectral lines indicated as being disturbed, and of those four lines we only know about one.

In that way it very soon became perfectly clear to those who were working at the sun, that in all these disturbances, or at all events in most of them, we were dealing to a large extent with lines not seen in our laboratories when dealing with terrestrial substances; this work went on till ultimately, thanks to the labours of Professor Young in America, we had a considerable list of lines coming from known and unknown substances which had been observed under these conditions in solar disturbances, and Professor Young was enabled to indicate the relative number of times these lines were visible. For instance, the lines which are most frequently seen under these conditions he tabulated as represented by the number 100, and of course the line which was least frequently seen would be represented by 1; and therefore from these so-called "frequencies" we got a good idea of the number of times we might expect to see any of these disturbance-lines when anything was going on in the sun.

It was this kind of work which made Tennyson write those very beautiful lines:

> "Science reaches forth her arms
> To feel from world to world".[1]

[1] And then he added:

> "and charms
> Her secret from the latest moon".

I mention this because Tennyson, whose mind was saturated with astronomy, had already grasped the fact that what had already been done was a small matter compared with what the spectroscope could do; and now the prophecy is already fulfilled, for by means of the spectroscopic examination of the light from the stars we can tell that some of them are double stars, that is to say, in poetic language, stars with attendant moons. Although we can thus charm the secret from each moon by means of the spectroscope, to see the moon it would require (in the case of β Aurigæ) a telescope not eighty feet long, but with an object-glass eighty feet in diameter, because the closer two stars are together the greater must be the diameter of the object-glass, independently of its focal-length and magnifying power.

DR. HILLEBRAND'S RESEARCHES ON URANINITE, 1888.

In this year Dr. Hillebrand, one of the officials in the Geological Department at Washington, was engaged upon the chemical examination of specimens of the mineral uraninite from various localities.

He dealt with crystals which he put in a vessel containing some sulphuric acid and water. He found that bubbles of gas were produced out of the crystal by means of the sulphuric acid. He collected this gas and came to the conclusion that it was nitrogen.

This result was new. He thus wrote about it :—

"In consequence of a certain observation" [the one I have just referred to] "and its results, an entirely new direction was given to the work, and its scope wonderfully broadened. This was the discovery of a hitherto unsuspected element in uraninite, existing in a form of combination not before observed in the mineral world."

It is not needful here to follow Dr. Hillebrand through all the painstaking and patient labour he cut out for himself to explain this anomalous behaviour. Needless to say he did not omit to employ the spectroscope to test the nature of the new gas.

His observations were thus described :—[1]

"In a Geissler tube under a pressure of ten millimetres and less, the gas afforded the fluted spectrum of pure nitrogen as brilliantly and as completely as was done by a purchased nitrogen tube. In order that no possibility of error might exist, the tube was then reopened and repeatedly filled with hydrogen, and evacuated till only the hydrogen lines were visible. When now filled with the gas and again evacuated, the nitrogen spectrum appeared as brilliantly as before, with the three bright hydrogen lines added."

On this paragraph I may remark that it has long been known that gases like nitrogen give us quite distinct spectra at different temperatures—one fluted, another containing

[1] "On the Occurrence of Nitrogen in Uraninite," *Bulletin*, No. 78, U.S. Geol. Survey, 1889-90, p. 55.

lines. Which of these we shall see in a tube will depend upon the pressure of the gas and the electric current used. The fluted spectrum of nitrogen is very bright and full of beautiful detail in the orange part of the spectrum; the line spectrum, on the other hand, is almost bare in that region.

It is important to note *that it so happened* that the pressure and electric conditions employed by Dr. Hillebrand enabled him generally to see the fluted spectrum. This however was not always the case. In an interesting letter to Professor Ramsay he writes (*Proc. Roy. Soc.*, vol. lviii., p. 81):—

"Both Dr. Hallock and I observed numerous bright lines on one or two occasions, some of which apparently could be accounted for by known elements—as mercury, or sulphur from sulphuric acid; but there were others which I could not identify with any mapped lines. The well-known variability in the spectra of some substances under varying conditions of current and degree of evacuation of the tube led me to ascribe similar causes for these anomalous appearances, and to reject the suggestion made by one of us in a doubtfully serious spirit, that a new element might be in question."

Dr. Hillebrand concludes his paper as follows:—

"The interest in the matter is not confined merely to a solution of the composition of this one mineral; it is broader than that, and the question arises: May not nitrogen be a constituent of other species in a form hitherto unsuspected and unrecognisable by our ordinary chemical manipulations? And, if so, other problems are suggested which it is not now in order to discuss."

D^3 AND OTHER UNKNOWN LINES IN NEBULÆ, 1890.

A negative of the nebula of Orion, taken at my observatory at Westgate-on-Sea in 1890, contains fifty-six lines, and of course by determining, as we have been able to do approximately, the wave-lengths—the positions of these lines in the spectrum—we can determine the exact light notes represented, and therefore the substances which produce them. In this spectrum of the nebula of the Orion were

lines of unknown origin exactly coinciding with those unknown lines which I have already referred to as having been seen in the sun's atmosphere. Some of the unknown lines in that atmosphere, those that we have not been able to see in our laboratories, are identical in position with some of the unknown lines in the nebula of Orion.

I may remark that as early as 1886 Dr. Copeland had discovered D^3 in the visible spectrum of the nebula, and in a letter to him I had suggested that another line he had recorded at 447 might be Lorenzoni's *f*; this he thought to be probable. The matter was set for ever at rest by the photograph which established the presence of 4471 and 4026 as well, already noted as a solar line.

Professor Campbell, of the Lick Observatory, obtained other photographs of the spectrum of the nebula some two or three years after mine was taken. In the following list of lines in my photograph an asterisk denotes that Campbell gives a line nearly in the same position. He recorded no line which did not appear on my photograph.

3896*
3888*
4011
4026*
4121*
4143*
4168
4390*
4472*
4716*
4924

5875·8 = D^3

THE SAME UNKNOWN LINES OCCUR IN THE STARS, 1892.

About the year 1890 I began the photography of stellar spectra at Kensington, with special reference to their classification on the basis of the chemical constituents established by their spectra. By 1892 several important results had been obtained, while the progress of this branch of science lately has been so considerable that any statement regarding the positions of lines, and therefore the

chemical origins of them, may be made with a considerable amount of certainty as depending upon very accurate work.

The various classes in which the stars have been classified by different observers according to their spectra are discussed elsewhere, but some of the more salient differences must be pointed out here; thus we have stars with many lines in their spectra, others with comparatively few. I will take the many-lined stars first.

The diagram (Fig. 8) represents the spectrum of Arcturus, a star the spectrum of which closely resembles that of the sun. In α Cygni we have another star with many lines, but here we note, when we leave the hydrogen on one side and deal with the other stronger lines, that there is little relation between the solar spectrum and these lines.

I next come to the stars with few lines: these are well represented by many of the chief stars in the Constellation of Orion. Bellatrix is given as an example (Fig. 9).

Then, I have next to say that in the photographs of the spectra of many stars, chiefly of those more or less like Bellatrix, we found the same lines which we have so far classified as unknown for the reason that in our laboratories we have not been able to get any lines which correspond with them. I again mention D^3, 4471 and 4026, previously noted as appearing both in the chromosphere and in the nebula of Orion.

But the thing is much more interesting even than this; not only these, but all the chief unknown lines appearing in the nebula of Orion are also found in these stars. And this is so absolutely true that there is no necessity to give a list of the unknown lines seen in Bellatrix; every one of them given in the nebula has found its place, and (so far) practically no others.

This of course marked a great development of the inquiry, and makes the question of the unknown lines more important than ever.

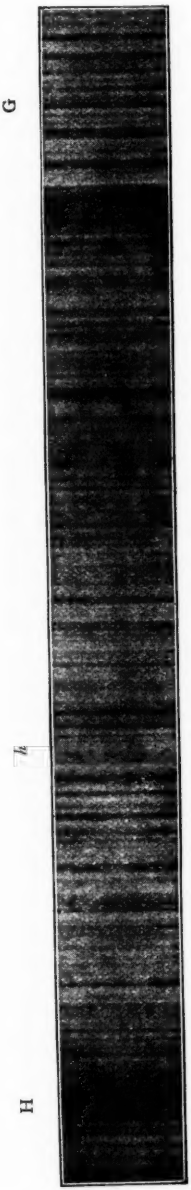


FIG. 8.—Spectrum of Arcturus between G and K.

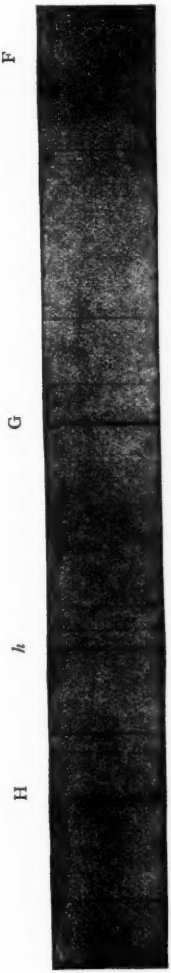


FIG. 9.—The spectrum of Bellatrix between H and F.

PHOTOGRAPHIC RESULTS DURING A SOLAR ECLIPSE, 1893.

A method which was first employed by Respighi and myself during the eclipse of 1871, was employed on a large scale and with great effect during the eclipse of 1893. The light proceeding from the luminous ring round the dark moon was made to give us a series of rings, representing each bright line seen by the ordinary method on a photographic plate. The observers this time were stationed in West Africa and in Brazil. The African station was up one of the rivers, not very far away from the town of Bathurst. The Brazilian station was near Para Curu. The same instrument which was previously referred to as used for obtaining photographs of the stars was sent to the African station in order that photographs of the eclipse of the sun might be taken on exactly the same scale as the photographs of the stars had been, so that the stellar and solar records in the photographs might be compared. The results obtained by Messrs. Fowler and Shackleton, who were in charge of the instruments at the two stations, will be gathered from the accompanying diagrams, Figs. 10 and 11.

We get more or less complete rings when we are dealing with an extended arc of the chromosphere, or lines of dots when any small part of it is being subjected to a disturbance which increases the temperature and, possibly, the numbers of the different vapours present.

The efficiency of this method of work with the dispersion employed turns out to be simply marvellous, and in securing such valuable and permanent records as these, we have done very much better than if we had contented ourselves with the style of observations that I have referred to as having been made in 1871.

As was expected the comparison between solar and stellar records thus rendered possible enabled a very great advance to be made.

On examining these eclipse records, we find that we have to do exactly with those unknown lines which had already been photographed in the stars and in the nebulæ.

As was to be expected we, of course, deal with the lines

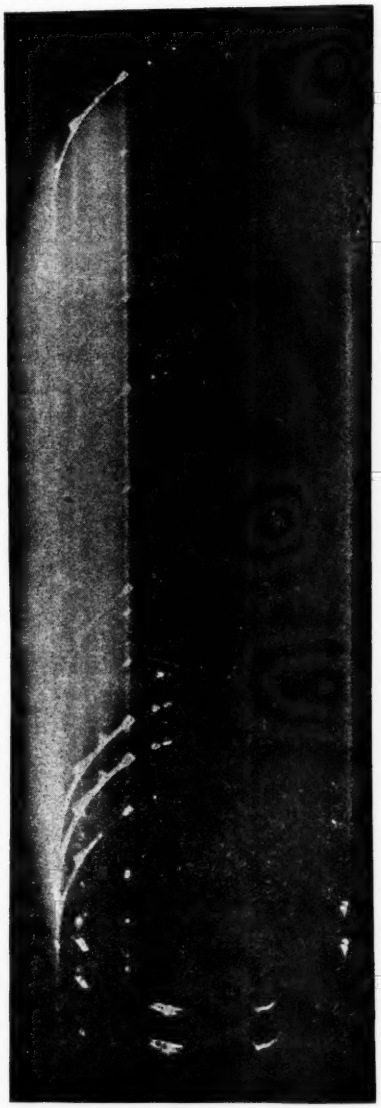


FIG. 10.—Untouched reproduction of photograph (African station) taken very shortly after the commencement of totality, the exposure being "instantaneous". At this phase of the eclipse a considerable arc of the chromosphere was visible, and its spectrum is therefore shown in addition to the spectrum of the higher reaches of some of the large prominences extending beyond the moon's limb. It will be seen that at H and K there are long arcs of chromosphere and prominences, the absent portions being of course obscured by the moon. One very small prominence is especially rich in lines.

recorded in the first observations of the solar disturbances, and chronicled in that table of Professor Young's to which I have already called attention; but the important thing is the marvellously close connection between eclipse- and star-spectrum photographs so far as the "unknown lines" are concerned.

Nearly all the lines given in the table on p. 259 as visible in the Nebula of Orion and afterwards found in Bellatrix, are also among the lines photographed during the eclipse.

DISCOVERY OF A TERRESTRIAL SOURCE OF HELIUM, 1895.

The year 1894 was made memorable by the announcement of the discovery by Lord Rayleigh and Professor Ramsay of a new gas called argon, and you know that the discovery was brought about chiefly in the first instance by the very accurate observations of Lord Rayleigh, who found that when he was determining the weight of air in the globe of a certain capacity, the weight depended upon the source from which he got the nitrogen.

From the nitrogen from atmospheric air he obtained one weight, and from that obtained by certain chemical processes he obtained another, and ultimately it was found that there was an unknown element which produced these results, these various changes in the weight, and as a consequence we had the 1895 discovery of argon.

Early in 1895 it struck Mr. Miers, of the British Museum, that it might be desirable to draw attention to the nitrogen which we have seen Dr. Hillebrand in 1888 obtaining from his crystal of uraninite; his observations, of course, were more in the mind of Mr. Miers than in the minds of the pure chemists. He therefore communicated with Professor Ramsay, who lost no time, because it was very interesting to study every possible source of nitrogen and see what its behaviour was in regard to the quantity of argon that it produced, and in the relation generally of the gas to the argon which was produced from it.

Professor Ramsay treated uraninite in exactly the same

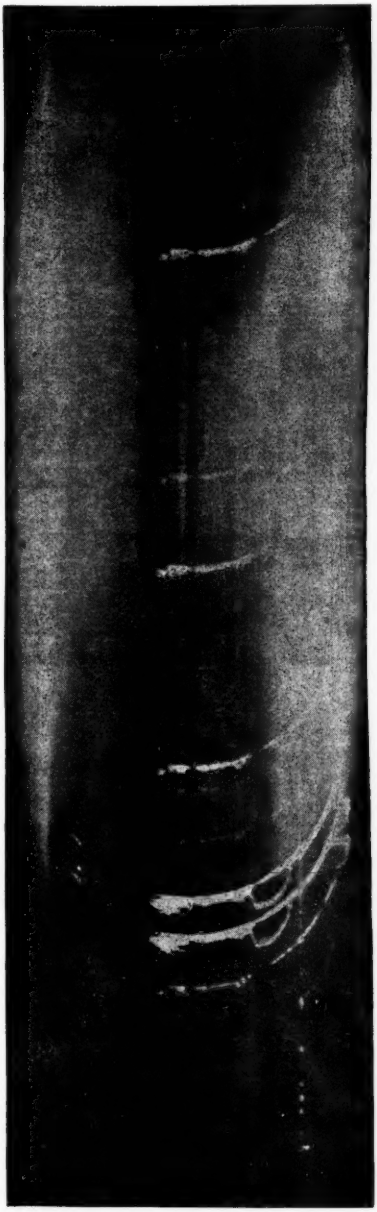


FIG. 11.—Photograph 21 (African station) taken shortly before the end of totality. A portion of the chromosphere on the other edge of the dark moon is now visible in addition to numerous prominences. It will be seen that one of the smallest prominences is rich in lines, and closely resembles that which appears in Fig. 11.

way that Dr. Hillebrand had done in 1888. The gas obtained as Dr. Hillebrand had obtained it was eventually submitted to a spectroscopic test, following Dr. Hillebrand's example. But here a noteworthy thing comes in.

It so happened that the pressure and electrical conditions employed by Professor Ramsay were so different from those used by Dr. Hillebrand that, although nitrogen was undoubtedly present, the fluted spectrum which, as I have previously stated, floods the orange part of the spectrum with luminous details, was absent. But still there was *something* there.

Judge of Professor Ramsay's surprise when he found that he got a bright orange line; that was the chief thing, and *not* the strong suggestion of the spectrum of nitrogen. Careful measurements indicated that the twenty-six-year-old helium had at last been run to earth, D^3 was at last visible in a laboratory. Professor Ramsay was good enough to send specimens of the tubes containing this gas round to other people, and he sent one of them to me.

I received Professor Ramsay's tube on 28th March, but it was not suitable for the experiments I wished to make.

On 29th March, therefore, as Professor Ramsay was absent from England, in order not to lose time I determined to see whether the gas which had been obtained by chemical processes would not come over by heating *in vacuo*, after the manner described by me to the Royal Society in 1879,[1] and Mr. L. Fletcher was kind enough to give me some particles of uraninite (bröggerite) to enable me to make the experiment.

This I did on 30th March, and it succeeded; the gas giving the yellow line came over, associated with hydrogen, in good quantity.

From 30th March onwards my assistants and myself had a very exciting time. One by one the unknown lines I had observed in the sun in 1868 were found to belong to the gas I was distilling from bröggerite; not only D^3 but 4923, 5017, 4471 (Lorenzoni's *f*), 6677 (the B C of Fig. 7), referred to previously, and many other solar lines, were all caught in a few weeks.

[1] *Roy. Soc. Proc.*, vol. xxix., p. 266.

But this was by no means all. The solar observations had been made by eye, and referred therefore to the less refrangible part of the spectrum, but I had obtained and studied hundreds of stellar photographs, so I at once proceeded to photograph the gas and compare its more refrangible lines with stellar lines.

Here, if possible, the result was still more marvellous. In the few-lined stars by 6th May I had caught nearly all the most important lines at the first casts of the spectroscopic net. Fig. 15, which includes some later results, will give an idea of the tremendous revelation which had been made as to the chemistry of some of the stages of star-life. I pointed out on 8th May that we had already "run home" the most important lines in the spectra of Group III. in which stars alone we find D^3 reversed.

These results enabled us at once to understand how it was that the "unknown lines" had been seen both in the sun's chromosphere and some nebulæ and stars. The gas obtained from the minerals made its appearance in the various heavenly bodies in which the conditions of the highest temperatures were present; and the more the work goes on, we find that this gas is really the origin of most, but certainly not of *all*, of the unknown lines which have been teasing astronomical workers for the last quarter of a century.

THE FIRST INVESTIGATIONS OF THE SPECTRUM OF THE GAS FROM CLEVEITE.

The dates of the papers communicated to the Royal Society recording the observations of the lines in the gas obtained from minerals which had been previously recorded are as follows :—

25th April, - - -	4471	4144	
8th May, - - -	667	4388	4026
9th May, - - -	3889		
28th May, - - -	7065		
29th May, - - -	5048	5016	4922

The lines at 667 and 5016 had been previously seen by Thalén (*Comptes Rendus*, 16th April, 1895).

Although the general distribution and intensities of the lines in the gases from bröggerite and cleveite sufficiently corresponded with some of the chief "unknown lines" in the solar chromosphere and some of the stars to render identity probable, it was necessary to see how far the conclusion was sustained by detailed investigations of the wave-lengths of the various lines.

EMPLOYMENT OF HIGH DISPERSION.

This was practically a separate branch of the work, as the observations had to be made in the observatory. Next I give here the observations relating to D^3, 4471.

The Orange Line, λ 5875·9.—Immediately on receiving from Professor Ramsay, on 28th March, a small bulb of the gas obtained from cleveite, a provisional determination of wave-length was made by Mr. Fowler and myself, in the absence of the sun, by micrometric comparisons with the D lines of sodium, the resulting wave-length being 5876·07 on Rowland's scale. It was at once apparent, therefore, that the gas line was not far removed from the chromospheric D_3, the wave-length of which is given by Rowland as 5875·98.

The bulb being too much blackened by sparking to give sufficient luminosity for further measurements, I set about preparing some of the gas for myself by heating bröggerite *in vacuo*, in the manner I have already described. A new measurement was thus secured on 30th March, with a spectroscope having a dense Jena glass prism of 60°; this gave the wave-length 5876·0.

On 5th April, I attempted to make a direct comparison with the chromospheric line, but though the lines were shown to be excessively near to each other, the observations were not regarded as final.

Professor Ramsay having been kind enough to furnish me, on 1st May, with a vacuum tube which showed the orange line very brilliantly, a further comparison with the chromosphere was made on 4th May. The observations were made by Mr. Fowler, in the third order spectrum of a grating having 14,438 lines to the inch, and the observing

telescope was fitted with a high power micrometer eye-piece; the dispersion was sufficient to easily show the difference of position of the D_3 line on the east and west limbs, due to the sun's rotation. Observations of the chromosphere were therefore confined to the poles.

During the short time that the tube retained its great brilliancy, a faint line, a little less refrangible than the bright orange one, and making a close double with it, was readily seen; but afterwards a sudden change took place, and the lines almost faded away. While the gas line was brilliant, it was found to be "the least trace more refrangible than D_3, about the thickness of the line itself, which was but narrow" ("Observatory Note Book"). The sudden diminution in the brightness of the lines made subsequent observations less certain, but the instrumental conditions being slightly varied, it was thought that the gas line was probably less refrangible than the D_3 line by about the same amount that the first observation showed it to be more refrangible. Giving the observations equal weight, the gas line would thus appear to be probably coincident with the middle of the chromospheric line, but if extra weight be given to the first observation, made under much more favourable conditions, the gas line would be slightly more refrangible than the middle of the chromosphere line.

Pressure of other work did not permit the continuation of the comparisons. In the meantime, Runge and Paschen announced (*Nature*, vol. lii., p. 128) that they also had seen the orange line of the cleveite gas to be a close double, neither component having exactly the same wave-length as D_3, according to Rowland.

They give the wave-length of the brightest component as 5878·883, and the distance apart of the lines as 0·323.

This independent confirmation of the duplicity of the gas line led me to carefully re-observe the D_3 line in the chromosphere for evidences of doubling. On 14th June observations were made by Mr. Shackleton and myself of the D_3 line in the third and fourth order spectra under favourable conditions; "the line was seen best in the fourth order, on an extension of the chromosphere or prominence

on the north-east limb of the sun. The D_3 line was seen very well, having every appearance of being double, with a faint component on the red side, dimming away gradually; the line of demarcation between the components was not well marked, but it was seen better in the prominence than anywhere else on the limb" ("Observatory Note Book").

It became clear, then, that the middle of the chromosphere line, as ordinarily seen, and as taken in the comparison of 4th May, does not represent the place of the brightest component of the double line, so that exact coincidence was not to be expected.

The circumstance that the line is double in both gas and chromosphere spectrum, in each the less refrangible component being the fainter, taken in conjunction with the direct comparisons which have been made, rendered it highly probable that one of the gases obtained from cleveite is identical with that which produces the D_3 line in the spectrum of the chromosphere.

Other observers have since succeeded in resolving the chromospheric line. On 20th June, Professor Hale found the line to be clearly double in the spectrum of a prominence, the less refrangible component being the fainter, and the distance apart of the lines being measured as 0·357 tenth-metres (*Ast. Nach.*, 3302).

The doubling was noted with much less distinctness in the spectrum of the chromosphere itself on 24th June. Professor Hale points out that Rowland's value of the wavelength (as well as that of 5875·924, determined by himself on 19th and 20th June) does not take account of the fact that the line is a close double.

Dr. Huggins, after some failures, observed the D_3 line to be double on 10th July (*Ast. Nach.*, 3302); he also notes that the less refrangible component was the fainter, and that the distance apart of the lines was about the same as that of the lines in the gas from cleveite, according to Runge and Paschen.

It may be added, that in addition to appearing in the chromosphere, the D_3 line has been observed as a bright line in nebulæ by Dr. Copeland, Professor Keeler and

others; in β Lyræ and other bright line stars; and as a dark line in such stars as Bellatrix, by Mr. Fowler, Professor Campbell and Professor Keeler. In all these cases it is associated with other lines, which, as I shall show presently, are associated with it in the spectra of the new gases.

The Blue Line, λ 4471·8.—A provisional determination on 2nd April of the wave-length of a bright blue line, seen in the spectrum of the gases obtained from a specimen of cleveite, showed that it approximated very closely to a chromospheric line, the frequency of which is stated as 100 by Young.

This line was also seen very brilliantly in the tube supplied to me by Professor Ramsay on 1st May, and on 6th May it was compared directly with the chromosphere line by Mr. Fowler. The second order grating spectrum was employed. The observations in this region were not so easy as in the case of D_3, but with the dispersion employed, the gas line was found to be coincident with the chromospheric one. In this case also, the chromosphere was observed at the sun's poles, in order to eliminate the effects due to the sun's rotation.

Besides appearing in the spectrum of the chromosphere, the line in question is one of the first importance in the spectra of nebulæ, bright line stars, and of the white stars such as Bellatrix and Rigel.

The Infra-red Line, λ 7065·5.—In addition to D_3 and the line at 4471·8, there is a chromospheric line in the infra-red which also has a frequency of 100, according to Young. On 28th May I communicated a note to the Royal Society stating that this line had been observed in the spectrum of the gases obtained from bröggerite and euxenite (*Roy. Soc. Proc.*, vol. lviii., p. 192), solar comparisons having convinced me that the wave-length of the gas line corresponded with that given by Young; and I added: "It follows, therefore, that besides the hydrogen lines all three chromospheric lines in Young's list which have a frequency of 100 have now been recorded in the spectra of the new gas or gases obtained from minerals by the distillation method".

M. Deslandres, of the Paris Observatory, has also

observed the line at 7065 in the gas obtained from the cleveite (*Comptes Rendus*, 17th June, 1895, p. 1331).

A great deal of work has been done upon these gases from other points of view than those which affect their cosmical relations, and perhaps I may be allowed next to refer to some of the results which have been obtained by myself.

HELIUM NOT CONNECTED WITH ARGON.

The first point is that the gas from the minerals contains no argon. Dr. Ramsay in his first experiments came to the conclusion that the spectra of argon and helium contained many common lines; indeed at first the observed coincidences were so remarkable that he came to the conclusion that the connection was so close that atmospheric argon contained a gas absent from the argon seen in his helium tube.

This statement was subsequently withdrawn, but the compound nature both of argon and helium was suggested by the fact that there were lines common to the two gases. These lines were in the red; one coincidence I found broke down with moderate dispersion, the other yielded subsequently to the still greater dispersion employed by Drs. Runge and Paschen. It may be also stated here that I have not found a single coincidence between argon and any line in the spectrum of any celestial body whatever. This happens, as everybody knows, also in the case of oxygen, nitrogen, chlorine, and the like.

THE CLEVEITE GAS A COMPOUND.

The first spectroscopic observations made it perfectly obvious that the gas as obtained from uraninite is a mixture of gases, that the gas which gives the yellow line is not an isolated one, but is mixed up with other gases which give other lines.

In May I wrote as follows:—[1]

"The preliminary reconnaissance suggests that the gas obtained from bröggerite by my method is one of complex origin.

[1] *Proc. R. S.*, lviii., p. 114.

"I now proceed to show that the same conclusion holds good for the gases obtained by Professors Ramsay and Clève from cleveite.

"For this purpose, as the final measures of the lines of the gas as obtained from cleveite by Professors Ramsay and Clève have not yet been published, I take those given by Crookes and Clève, as observed by Thalén.

"The most definite and striking result so far obtained is that in the spectra of the minerals giving the yellow line I have so far examined, I have never once seen the lines recorded by Crookes and Thalén in the blue. This demonstrates that the gas obtained from certain specimens of cleveite by chemical methods is vastly different from that obtained by my method from certain specimens of bröggerite, and since, from the point of view of the blue lines, the spectrum of the gas obtained from cleveite is more complex than that of bröggerite, the gas itself cannot be more simple.

"Even the blue lines themselves, instead of appearing *en bloc*, vary enormously in the sun, the appearances being

4922 (4921·3) = thirty times
4713 (4712·5) = twice.

"These are not the only facts which can be adduced to suggest that the gas from cleveite is as complex as that from bröggerite."

It is seen that quite early in the inquiry we had not only spectroscopic evidence in the laboratory which was complete in itself, but that the case was greatly strengthened when the behaviour of the various lines in the sun and stars was also brought into evidence.

In the first case we had the laboratory separation of D_3 from the lines 5048, 5016, and 4922.

Later on in the same month I showed that the lines at D_3 and 447 behaved in one way, and that at 667 behaved in another.

In order to test this view I made some observations based on the following considerations:—

(1) In a simple gas like hydrogen, when the tension of the electric current given by an induction coil is increased

by inserting first a jar and then an air-break into the circuit, the effect is to increase the brilliancy and the breadth of all the lines, the brilliancy and breadth being greatest when the longest air-break is used.

(2) Contrariwise, when we are dealing with a known compound gas; at the lowest tension we may get the complete spectrum of the compound without any trace of its constituents, and we may then, by increasing the tension, gradually bring in the lines of the constituents, until, when complete dissociation is finally reached, the spectrum of the compound itself disappears.

Working on these lines the spectrum of the spark at atmospheric pressure passing through the gas or gases, distilled from bröggerite, has been studied with reference to the special lines C (hydrogen), D_3, 667, and 447.

The first result is that all the lines do not vary equally as they should do if we were dealing with a simple gas.

The second result is that at the lowest tension 667 is relatively more brilliant than the other lines; on increasing the tension C and D_3 considerably increase their brilliancy, 667 relatively and absolutely becoming more feeble, while 447, seen easily as a narrow line at low tension, is almost broadened out into invisibility as the tension is increased in some of the tubes, or is greatly brightened as well as broadened in others (Fig. 12).

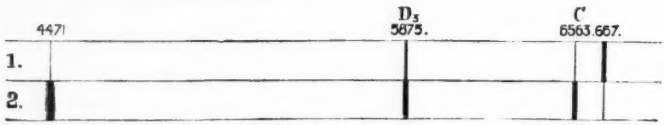


FIG. 12.—Diagram showing changes in intensities of lines brought about by varying the tension of the spark. 1. Without air-break. 2. With air-break.

The above observations were made with a battery of five Grove cells; the reduction of cells from 5 to 2 made no difference in the phenomena except in reducing their brilliancy.

Reasoning from the above observations it seems evident that the effect of the higher tension is to break up a compound or compounds, of which C, D_3, and 447 represent constituent elements; while, at the same time, it would

appear that 667 represents a line of some compound which is simultaneously dissociated.

The unequal behaviour of the lines has been further noted in another experiment, in which the products of distillation of bröggerite were observed in a vacuum tube and photographed at various stages. After the first heating D_3 and 4471 were seen bright, before any lines other than those of carbon and hydrogen made their appearance. With continued heating 667, 5016, and 492 also appeared, although there was no notable increase of brightness in the yellow line; still further heating introduced additional lines, 5048 and 6347.

These changes are represented graphically in the following diagram (Fig. 13).

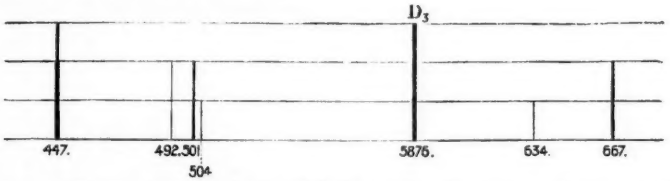


FIG. 13.—Diagram showing order in which lines appear in spectrum of vacuum tube when bröggerite is heated.

It was recorded further that the yellow line was at times dimmed, while the other lines were brightened.

In my second note, communicated to the Royal Society on the 8th May, I stated that I had never once seen the lines recorded by Thalén in the blue, at λ 4922 and 4715.

It now seems possible that their absence from my previous tubes was due to the fact that the heating of the minerals was not sufficiently prolonged to bring out the gases producing these lines.

It is perhaps to the similar high complexity of the gas obtained from cleveite that the curious behaviour of a tube which Professor Ramsay was so good as to send me, must be ascribed. When I received it from him the glorious yellow effulgence of the capillary while the current was passing was a sight to see. But after this had gone on for some time, while the coincidence of the yellow line with D_3 of the chromosphere was being inquired into, the luminosity of the tube was considerably reduced, and the colours

in the capillary and near the poles were changed. From the capillary there was but a feeble glimmer, not of an orange tint, while the orange tint was now observed near the poles, the poles themselves being obscured by a coating on the glass of brilliant metallic lustre.

After attempting in vain for some time to determine the cause of the inversion of D_3 and 447 in various photographs I had obtained of the spectra of the products of distillation of many minerals, it struck me that these results might be associated with the phenomena exhibited by the tube, and that one explanation would be rendered more probable if it could be shown that the change in the illumination of the tube was due to the formation of platinum compounds, platinum poles being used. On 21st May I accordingly passed the current and heated one of the poles, rapidly changing its direction to assure the action of the negative pole, when the capillary shortly gave a very strong spectrum of hydrogen, both lines and structure. A gentle heat was continued for some time, and apparently the pressure in the tube varied very considerably, for as it cooled the hydrogen disappeared and the D_3 line shone out with its pristine brilliancy. The experiment was repeated on 24th May, and similar phenomena were observed.

Some little time after[1] Professors Runge and Paschen, from an entirely different standpoint, arrived at exactly the same conclusion.

The employment of exposures extending over seven hours has given a considerable extension in the number of lines, and the bolometer has been called in to investigate lines in the infra-red; better still, they have employed well-practised hands in searching for series of lines. Operating by chemical means upon a crystal of cleveite free from any other mineral, they have obtained a product so pure that from these series there are no outstanding lines. Very great weight, therefore, must be attached to their conclusions.

As a result of their investigations Drs. Runge and Paschen stated that the gas given off even by a pure crystal

[1] *Nature*, 26th September, 1895.

of cleveite is not simple. In their view the mixture consists of two constituents.

This conclusion was arrived at from the following considerations. "The wave-lengths λ of the lines belonging to the same series are always approximately connected by a formula somewhat similar to Balmer's—

$$1/\lambda = A - B/m^2 - C/m^4.$$

A determines the end of the series towards which the lines approach for high values of m, but does not influence the difference of wave-numbers of any two lines. B has nearly the same value for all the series observed, and C may be said to determine the spread of the series, corresponding intervals between the wave-numbers being larger for larger values of C. As B is approximately known two wave-lengths of a series suffice to determine the constants A and C, and thus to calculate approximately the wave-lengths of the other lines. It was by this means that we succeeded in disentangling the spectrum of the gas in cleveite, and showing its regularity.

"In the spectrum of many elements two series have been observed for which A has the same value, so that they both approach to the same limit. In all these cases the series for which C has the smaller value, that is to say, which has the smaller spread, is the stronger of the two. In the spectrum of the gas in cleveite we have two instances of the same occurrence. One of the two pairs of series, the one to which the strong yellow double line belongs, consists throughout of double lines whose wave-numbers seem to have the same difference, while the lines of the other pair of series appear to be all single. Lithium is an instance of a pair of series of single lines approaching to the same limit. But there are also many instances of two series of double lines of equal difference of wave-numbers ending at the same place as sodium, potassium, aluminium, etc. There are also cases where the members of each series consist of triplets of the same difference of wave-numbers, as in the spectrum of magnesium, calcium, strontium, zinc, cadmium, mercury. But there is no instance of an element whose spectrum contains two pairs of series ending at the same place. This

suggested to us the idea that the two pairs of series belonged to different elements. One of the two pairs being by far the stronger, we assume that the stronger one of the two remaining series belongs to the same element as the stronger pair. We thus get two spectra consisting of three series each, two series ending at the same place, and the third leaping over the first two in large bounds and ending in the more refrangible part of the spectrum. This third series we suppose to be analogous to the so-called principal series in the spectra of the alkalis, which show the same features. It is not impossible, one may even say not unlikely, that there are principal series in the spectra of the other elements. But so far they have not been shown to exist.

"Each of our two spectra now shows a close analogy to the spectra of the alkalis.

"We therefore believe the gas in cleveite to consist of two, and not more than two, constituents."

To the one containing the line D_3, which I discovered in 1868, the name helium remains; the other for the present we may call "gas X".[1]

The chief lines of these two constituents are as follows, according to Runge and Paschen, the wave-lengths being abridged to five figures.

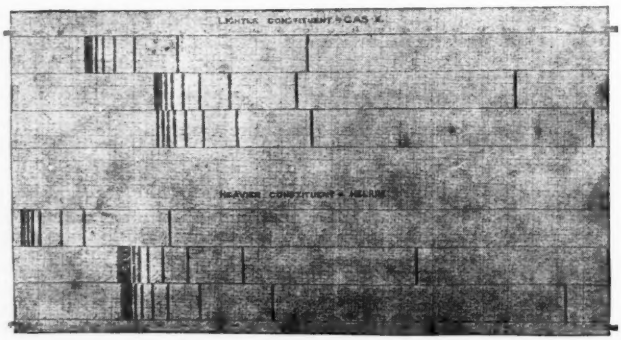


FIG. 14.—Runge and Paschen's results suggesting that cleveite gives off two gases, each with three series of lines.

[1] In the many comparisons I had to make, I soon found the inconvenience of not having a name for the gas which gave 667, 501 and other lines. When, therefore, Professors Runge and Paschen, who had endorsed

HELIUM.

Principal Series.	1st Subordinate Series.	2nd Subordinate Series.
2663·3	3456·9 ?	3481·6
2677·2	3461·4 ?	3490·8
2696·2	3466·0	3502·5
2723·3	3471·9	3517·5
2763·9	3479·1	3537·0
2829·2	3487·9	3563·1
2945·2	3498·8	3733·0
3187·8	3512·6	3867·6
3888·8	3530·6	4121·0
	3554·6	4713·3
	3587·4	7065·5
	3634·4	
	3705·1	
	3819·8	
	4026·3	
	4471·6	
	5875·8	

GAS X.

Principal Series.	1st Subordinate Series.	2nd Subordinate Series.
3176·6	3756·2	3770·7
3196·8	3768·9	3787·6
3211·6	3785·0	3838·2
3231·3	3805·9	3878·3
3258·3	3833·7	3936·0
3296·9	3872·0	4024·1
3354·7	3926·7	4169·1
3447·7	4009·4	4437·7
3613·8	4143·9	5047·8
3964·9	4388·1	7281·8
5015·7	4922·1	
	6678·4	

my results, and had extended them, called upon me, I thought it right to suggest to them that, sinking the priority of my own results, we should all three combine in suggesting a name. Professor Runge (under date 20th October) wrote me: "The inference that there are two gases is a spectroscopical one, being based on the investigation of the 'series'. Now, though we think this basis quite sound, we must own that the conclusion rests on induction. . . . For this reason we do not want to give a name to 'gas X'." I have so far suggested no name, though Orionium and Asterium have been in my mind.

More recently Professor Ramsay has abandoned his view of the simple nature of the cleveite gas, and states that from his experiments "there appears ground for the supposition that helium is a mixture".[1]

THE EXISTENCE OF THE NEW GASES IN CELESTIAL BODIES.

And now comes the great revelation, and it is this. The majority of the lines classed as unknown in the spectra of the Orion nebula, stars of Group III. and the sun are really due to the cleveite gases.

The following table sets this result out. It will be seen that of seventeen unknown lines, twelve have been run to earth.

COMPARISON OF UNKNOWN (PREVIOUS TO HE. AND X) LINES IN ORION NEBULA AND BELLATRIX.

Orion Nebula.		Bellatrix and Eclipse, 1893.	Origin.
Campbell.	Lockyer.		
3869	*3869 (7)	†3867·5 (Falls on He.)	He.
3889	3888 (7)	3888	He.
—	4011 (3)	4009 (8)	X
4026	4026 (5)	4026 (10)	He.
—	4042 (1)	4041 (3)	Still Unknown
4067	4068 (3)	4070 (3)	Still Unknown
4121	4121 (1)	4121·3 (7)	He.
4143	4143 (1)	4144 (8)	X
—	4168 (1)	4169 (5)	X
4265	4270 (3)	4268 (7)	Still Unknown
4389	4390 (3)	4389 (8)	X
4472	4472 (7)	4472 (10)	He.
—	4540 (3)	4541 (1)	Still Unknown
—	4628 (3)	4630 (3)	Still Unknown
4714	4716 (3)	4715 (5)	He.
—	*4924 (5)	†4922·1 (8)	X
5874	5875·8	5875·8	D_3 He.

* Between these λλ there are forty-two lines in the Orion photograph of which six are known other than He. and X.

† Between these λλ there are forty-five lines in the Bellatrix photograph of which five are known other than He. and X.

The following tables give the complete list of lines and the celestial body in which they have been traced.

[1] *Nature*, vol. liii., p. 598.

In the tables, under "sun," C, followed by a number, indicates the frequency as given by Young; E indicates the lines photographed during the eclipse of 1893. Under "star or nebula" the references are to the tables given in my memoir on the nebula of Orion (*Phil. Trans.*, vol. clxxxvi., 1895, p. 86 *et seq.* N = Nebula of Orion).

HELIUM.

11220.	Sun.	Star or Nebula.
3889	C E	N. III. γ
3188		
2945		
2829		
2764 *		
2723		
2696		
2677		
5876	C 100 E	
4472	C 100 E	
4026	C 25 E	
3820	E	α Cygni
3705		
3634		
3587		
3555		
3513		
3499 *		
3488		
3479		
3472		
3466		
3461		
7066	C 100	
4713	C 2 E	
4121	E	N. α Cygni
3868	?	
3777	E	Bellatrix
3652		
3599		
3567		
3537		
3517 *		
3503		
3491		
3482		

* Means that these lines are out of the range of my observations.

GAS X.

	Sun.	Star or Nebula.
5016	C 30 E	
3965	?	III. γ
3614	E	
3448		
3355		
3297 *		
3258		
3231		
3213		
6678	C 25	
4922	C 30 E	
4388	E	N. III. γ
4144	E	III. γ
4009		III. γ
3927		Bellatrix
3872		Bellatrix
3833	E	Hid by H. line
3806		Bellatrix
3785*		
7282		
5048	C 2	
4438		Bellatrix
4169		Bellatrix
4024	?	N. III. γ
3936	Hid in K.	
3878	C E	α Cygni
3838	C E	α Cygni
3803*		

* Means that these lines are out of the range of my observations.

The annexed reproduction of a photograph of Bellatrix will show how striking has been the result of the discovery so far as stellar spectra are concerned.

Hydrogen, helium and gas X are thus proved to be those elements which are, we may say, completely represented in the hottest stars and in the hottest part of the sun that we can get at. Here then, in 1895, we have abundant confirmation of the views I put forward in 1868 as to the close connection between helium and hydrogen.

EFFECTS OF DIFFUSION.

A diffusion experiment described in their paper enabled Messrs. Runge and Paschen to go a stage farther, and to

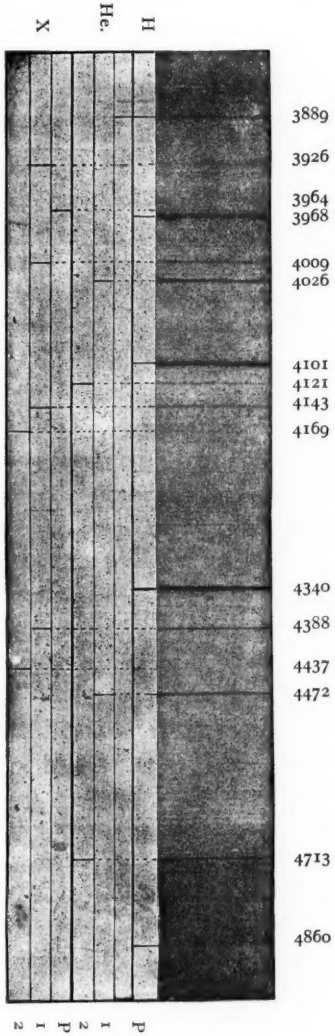


FIG. 15.—The spectrum of Bellatrix showing the lines of hydrogen and those which have been traced to the gases obtained from minerals.

announce that of their two constituents the gas-giving D^3 was the heavier one. They also add :—

"From the fact that the second set of series is on the

whole situated more to the refrangible part of the spectrum, one may, independently of the diffusion experiment, conclude that the element corresponding to the second set is the heavier of the two".

As they themselves pointed out, however, the result was not final, because the pressures were not the same. I have recently made some experiments in which the pressures remain the same.

An U tube was taken, and at the bend was fixed a plaster of Paris plug about 1·5 c.m. thick; in one of the limbs two platinum wires were inserted. The plug was saturated with hydrogen to free it from air; the tube was then plunged into a mercury trough, and fixed upright with the limbs full of mercury. Into the leg (A) with the platinum wires a small quantity of hydrogen was passed, and as soon after as possible another small quantity of a mixture of helium and hydrogen from samarskite was put up the other limb (B) of the U tube.

Immediately after the helium was passed into the limb (B) spectroscopic observations were made of the gas in the limb (A); D_3 was already visible, and there was no trace of 5015·7. This result seems to clearly indicate that if a true diffusion of one constituent takes place, the component which gives D_3 is lighter than the one which gives the lines at wave-length 5015·7.

Although this result is opposed to the statement made by Runge and Paschen, it is entirely in harmony with the solar and stellar results.

In support of this I may instance that of the cleveite lines associated with hydrogen in the chromosphere and the stars of Group III. γ; those allied to D_3 are much stronger than those belonging to the series of which 5015·7 forms part.

MINERALS EXAMINED.

So far I have worked upon some seventy minerals, and I have found the orange line in sixteen.

The following are the minerals, etc., which have been

investigated; those which give the D_3 line being marked with an asterisk :—

*Æschynite.
Almandine.
Anglesite.
Anhydrite.
Augite.
Barytes.
*Bröggerite.
Bronzite.
Calco-uranite.
Cassiterite.
Celestine.
Chalk.
Charnockite.
Chromite.
*Cleveite.
Columbite.
Crocidolite.
Cupro-uranite.
*Eliasite.
Enstatite.
*Euxenite.
*Fergusonite.
Franklinite.
Gadolinite.
Gahnite.
Geikielite.
Gneiss.
Granite.
Graphite.
*Gummite.
Hæmatite.
*Hielmite.
Hornblende.
Hypersthene.
Ilmenite.
Iridosmine.
Kielhanite.
Kyanite.
Ludwigite.
Magnesium.
Magnetite.
Manganese Nodule.
Minium.
*Monazite.
Obsidian.
Olivine.
Olivine-Enstatite.
*Orangeite.
Orthite.
Pitchblende.
Plumbic Ochre.
*Polycrase.
*Pyrochlore.
Quartz.
Red Clay.
Rhodonite.
*Samarskite.
Schorlomite.
Sphene.
Staurolite.
Thorite.
*Thoro-gummite.
*Uraninite.
Uranocircite.
Uranophane.
Wulfenite.
Wolfram.
Xenotine.
*Yttro-Gummite.

J. NORMAN LOCKYER.

INSULAR FLORAS.

PART VI. (A).

IN the preceding articles I have briefly reviewed the literature relating to Insular Floras which has appeared during the last decade, and I have extracted therefrom the principal or most interesting facts, which I have given with some comments of my own. That I have been able to do this with some profit is largely due to the advantages I have enjoyed through the kindness of the Director of the Royal Gardens, Kew. Since the publication in 1885 of my first essay on this subject, in the *Botany of the Voyage of H.M.S. "Challenger,"* all or nearly all collections of insular plants received at Kew have passed through my hands for determination and reporting on; and I have also been favoured with many notes and criticisms by travellers and other persons interested in plant distribution. I propose therefore to enter into a short recapitulation and discussion of the main facts thus accumulated; but before doing so I will refer to some more or less important contributions to the subject that have come to light during the progress of the present series of articles.[1]

It will be convenient to take the islands in the same geographical order previously followed (1), beginning with Polynesia.

There are some interesting recent contributions to the flora of Polynesia, taking the designation in its widest sense; but no one has yet attempted to bring together what is known, or ascertainable from materials preserved in herbaria, of the vegetation of the numerous small coral islands and groups of islands, more or less recently annexed by, or taken under the protection of, Great Britain. This the writer is engaged upon, and some particulars thus acquired may be

[1] A review of the additional literature having extended beyond what was expected, the discussion referred to will form the subject of a concluding article.

utilised here in dealing with the literature of the subject. Some years ago Mr. J. T. Arundel delivered a lecture at San Francisco, before the Geographical Society of the Pacific, on the Phœnix Group and other islands of the Pacific, and he has since published it (2) with additional notes. Mr. Arundel writes from actual experience, having visited a large number of the most remote islets of the Pacific and collected samples of their scanty floras, which were determined for him at Kew, where the specimens are preserved. Unfortunately several of the names of the plants in his list have undergone such a transformation as to be almost unrecognisable.

Besides the Phœnix Group, which was under his personal control, Mr. Arundel visited such out-of-the-way islands as Starbuck, Caroline (not the Caroline Group), Fanning, Malden, Palmerston and Ducie. Mr. Arundel describes Starbuck and Caroline Islands as examples of two kinds of very small islands common in the Pacific, though not confined to it. The former represents those consisting of an unbroken mass which is treeless, and indeed almost devoid of vegetation; and the latter is a typical coral atoll, consisting of a ring of islets encircling a central lagoon, and supporting a relatively luxuriant vegetation. Starbuck is very scantily furnished with vegetation, only about half a dozen species being represented. The principal plants are *Lepidium piscidium* and *Sida fallax;* both of wide range in Polynesia. Caroline Island claims a little more attention, because its history, position, conformation, meteorology, botany and zoology have been very fully worked out and illustrated. In 1883 this island was selected by the Americans, by the British, and by the French as the most suitable spot for observing the total eclipse of the sun. The American party was relatively numerous, and they drew up a somewhat elaborate report (3), illustrated chiefly by prints from photographs taken by the two gentlemen constituting the English party. These illustrations give an excellent idea of the form and vegetation of an atoll, including a bird's eye view, which enables us, better than any description could, to realise its smallness and isolation. Caroline Island

is situated in almost exactly 150° W. longitude and 10° S. latitude, and is distant, according to Mr. Arundel, about 400 miles from Tahiti, the nearest island of considerable size—say a third larger than the Isle of Wight; and 420 from Starbuck. Although in most parts well clothed with vegetation, this vegetation consists of very few, perhaps not more than twenty, species of vascular plants. Several others now exist, either as the remains of cultivation or accidental introduction; and the abundance of the cocoanut palm is due to planting, which has now been in operation for some years. Whether the cocoanut existed in the island on the first advent of man there is no evidence to show; but there are trees of other kinds of large size, as depicted and described in the report referred to. They are: *Calophyllum Inophyllum* (Guttiferæ), *Morinda citrifolia* (Rubiaceæ), *Cordia subcordata* (Borragineæ), *Pisonia grandis* (Nyetaginaceæ), and a screw pine, probably the widely spread *Pandanus odoratissimus*. One of the illustrations is a most effective representation of a group of screw pines. The *Cordia* is perhaps the commonest tree, and is most conspicuous, having a spreading crown with branches down to the ground. *Pisonia grandis* is described as forty or fifty feet high, with a trunk four feet in diameter; dimensions one would hardly have expected. I have drawn somewhat freely from this report, because it is by far the most instructive known to me.

A more recent contribution to island literature by Mr. C. M. Woodford (4) is equally deserving of attention, though wanting illustrations. It deals with the Gilbert Archipelago, one of the most remarkable of the numerous groups in the Eastern Pacific. There are sixteen islands, not counting the islets of the atolls, forming a chain, trending from north-west to south-east and extending from about 3° north to 3° south latitude in 173° to 177° east longitude. Eleven out of the sixteen are of atoll formation, and the largest of them is little more than twenty miles long and twenty feet high in the highest part. They are mostly inhabited, and the population half a century ago was estimated at 50,000, though it has since dwindled down to

probably a quarter of that number. The presence of so large a population must have had some modifying influence on the vegetation; yet not to the extent that might have been expected, because there is little cultivation, the natives living largely on fish, with which the waters swarm. Mr. Woodford says: "The islands are clothed from end to end with a dense growth of cocoanut palms and other vegetation, and present a beautiful appearance when approaching from the sea. The reefs and lagoons teem with fish, thus enabling the islands to support a population which for their land area was at one time equalled in no part of the world."

Mr. Woodford gives a list of the plants compiled from observations on the islands he visited, which he believes is nearly complete. As I am able to supplement it by a few additional species in the Kew Herbarium, chiefly collected by the Rev. Mr. Whitmee, and also to supply specific names in some cases where he gives only the generic, I will give a list of all the vascular plants known to inhabit the group, as a sample of the typical coral island flora. *Calophyllum Inophyllum* (Guttiferæ), *Sida fallax* (Malvaceæ), *Triumfetta procumbens* (Tiliaceæ), *Tribulus cistoides* (Zygophyllaceæ), *Pemphis acidula* (Lytheraceæ), *Rhizophora mucronata* (Rhizophoraceæ), *Guettarda speciosa* and *Morinda citrifolia* (Rubiaceæ), *Scævola Kœnigii* (Goodeniaceæ), *Tournefortia argentea* (Boraginaceæ), *Pisonia inermis* and *Boerhaavia diffusa* (Nyctaginaceæ), *Euphorbia Atoto?* (Euphorbiaceæ), *Ficus tinctoria* (Moraceæ), *Crinum pedunculatum?* (Amaryllidaceæ), *Cocos nucifera* (Palmaceæ), *Pandanus odoratissimus* (Pandanaceæ), *Fimbristylis glomerata* (Cyperaceæ), *Lepturus repens* (Gramineæ), and *Polypodium Phymatodes* (Filices)—just a score of species, it will be seen, belonging to as many different genera, and to eighteen different natural orders of the most diverse habit and structure. They are almost without exception plants of general distribution in tropical oceanic islands and on the sea-shores of the continents. The majority of them indeed inhabit the smaller remote islands of the tropical parts of the Indian Ocean. I will only add here that their seeds are such as

are transported by oceanic currents, birds, and winds, without destroying their vitality. In another article I propose discussing these agents of dispersal in some detail. The absence from the above list of the two largest natural orders—Leguminosæ and Compositæ—may cause some surprise, especially as the seeds of many of the former bear long immersion in salt water with impunity, and the pappose achenes of the latter are often, it is assumed, conveyed long distances by wind. Leguminosæ are rare in all oceanic islands, both coral and volcanic; but Compositæ, on the other hand, are characteristic of many volcanic islands, the Galapagos and St. Helena, for example.

The distribution of the plants of the Tonga or Friendly Islands has been worked out by the writer (5), and a few of the most interesting facts may be repeated here. This group lies to the south-east of Fiji, between 18° and 23° south latitude, and 173° and 176° west longitude, and comprises both volcanic and coral islands; some of the former being considerably larger than those of the Gilbert Group, and rise to altitudes of 500 to 3000 feet. Fuller information on the geology of the islands will be found in an article (6) by Mr. J. J. Lister. But although the Tonga Islands are considerably larger than the Gilbert Islands, it is more in land area and altitude than external dimensions, and it is due partly to the absence of central lagoons. Tongatabu in the south, the largest of the group, is about twenty-two miles in its greatest length, and is composed entirely of coral limestone. This island is the best known botanically; but Mr. J. J. Lister, whose collections were worked out for my paper referred to above, thoroughly explored the neighbouring smaller, though more elevated, Eua, which gave a considerable number of additional species. Since the publication of my paper, Kew has acquired a collection of dried plants made by Mr. C. S. Crosby in the Vavau cluster in the north. This collection has not yet been thoroughly worked out, but although it doubtless contains some additions, they will not be of a character to modify what has been written respecting the affinities of the flora of the whole group. The total num-

ber of assumed indigenous species of vascular plants in my enumeration is 290, whereof 246 have a westward, and 220 have an eastward extension in Polynesia; 138 are Australasian (Australia, New Zealand and outlying islands), 162 are Malayan, and at least 150 have a wider range either in the Old or New World, or in both. From the foregoing figures it will be seen that the flora of the Tonga Islands is largely composed, like the very small one of the Gilbert Islands, of species of wide distribution. Indeed no genus is peculiar to the group, and only ten species so far as our present knowledge goes are endemic, and a more complete exploration of the Fiji Islands and other neighbouring groups may reduce this number. The 290 species of the Tongan flora represent no fewer than 202 genera and seventy-nine natural orders out of the 202 recognised in Bentham and Hooker's *Genera Plantarum*. The proportions are 2·55 genera to an order, and 1·43 species to a genus in the Tongan flora. In the flora of the world the proportions I obtained by a very rough calculation are 37·50 genera to an order, and 12·65 species to a genus. Taking the number of Tongan species (138) which extend to Australasia, one might overestimate the affinities, because, as a matter of fact, a large proportion of these species have a wide range. Indeed only a dozen species have decidedly Australasian connections. These are: *Melicytus ramiflorus*, *Ratonia stipitata*, *Metrosideros polymorpha*, *Jasminum simplicifolium*, *Hoya australis*, *Ipomœa congesta*, *Pisonia inermis*, *Peperomia leptostachya*, *Euphorbia Sparmannii*, *Ficus aspera*, *Podocarpus elata* and *Pteris comans*. It will be perceived that the connections are specific rather than generic. But the most significant facts brought out in the paper under consideration are two, namely, the large proportion of species—upwards of a third—peculiar to Polynesia, and the strongly Malayan character of the flora, generally, of the Tonga, Fiji and Samoa Islands.

Several additional small contributions to the flora of the Solomon Islands have appeared (7), including some highly interesting novelties collected by the officers of H.M.S. *Penguin*, and the Rev. R. B. Comins. Excellent

photographs of the singular new genus *Sararanga* (Pandanaceæ) have been received at Kew, as well as ripe fruit in spirit, which will enable me to add to my published description, though not to complete it, because the male inflorescence is still unknown. Two species of *Begonia*, an *Oxymitra* (Anonaceæ) with flowers nearly nine inches long, a singular *Tabernæmontana* having a twisted fruit, and the anomalous genus *Lophopyxis* (8) are among the latest additions to the flora of the Solomon Islands. The last is doubtingly placed in the Euphorbiaceæ by Sir Joseph Hooker, and it has since been twice described (9 and 10), and placed in different natural orders, namely, *Combretopsis* (Olacineæ) and *Treubia* (Saxifragaceæ). There are two or three very closely allied species or races inhabiting Malacca, Ceram, New Guinea, and the Solomon Islands. I may refer in passing to a zoological paper (11) in which the author puts forward the theory of a former connection of the Solomon, Fiji, New Hebrides, Loyalty, New Caledonia, Norfolk and New Zealand Islands with New Guinea, but not with Australia. That there was, in the remote past, a greater land area in this region seems highly probable, but the relationships are so complex that fuller data are required to afford a solution of the problem. The present flora of Lord Howe Island, described a few pages forward, does not favour Mr. Hedley's views in their entirety on this point.

In my reference to the flora of Christmas Island (12) I overlooked a paper that supplemented mine to some extent (13), especially in relation to the vegetation.

Dr. Trimen (14) has published two more volumes of his admirable flora of Ceylon, bringing it down to the end of the Balanophoraceæ, following the arrangement of Bentham and Hooker's *Genera Plantarum*. The same author has drawn up a provisional list (15) of Maldive plants; the first, I believe, that has appeared. As might be expected there is no endemic element, and the vegetation is an assemblage of the ubiquitous coral island plants and weeds of cultivation. Dr. Trimen makes no mention of the *Cocos maldivica* or Coco-de-mer (*Lodoicea seychellarum*);

but, although it is improbable that this palm ever grew in the Maldive Islands, something yet remains to be done to complete its history. John de Barros, a Portuguese author, is thus quoted (16) by the writer of an article on these islands :—

"Their productions he also enumerates minutely, especially the coconut, both of the ordinary kind and of that called coco-de-mer, almost peculiar to the Seychelles, the seed of which appears to have been borne thence to the Maldivas by the currents of the ocean".

Since the publication of my notes on the flora of New Zealand and the outlying islands (17) several interesting papers on the subject have appeared, though there is only one of sufficient importance to call for more than brief mention. But first the minor ones. Mr. F. Kirk is the author (18) of a series of monographs treating of the genera *Gentiana*, *Colobanthus*, and *Gunnera*, as represented in the New Zealand region, besides descriptions of a number of new species belonging to various natural orders. The forms of *Gentiana* are numerous, and the species exceedingly difficult of delimitation. Kirk defines ten species, and about half of them comprise several varieties. They are spread all over New Zealand, except the extreme north, and they extend to the Chatham, Antipodes, Auckland and Campbell Islands; but hitherto no species has been found in Macquarie Island, the southernmost of these islands. They chiefly inhabit the mountains, in alpine and subalpine situations, and the sea-coast; four out of the ten, it is stated, not being found out of the reach of the sea-spray. They all belong to one group, characterised by having pentamerous flowers, unappendaged corollas, and versatile anthers. White is the prevailing colour of all the species, though some of them occasionally exhibit various shades, mostly dull, of red, purple, and violet, and more rarely a pale yellow. This is in direct contrast to the behaviour of the northern species, speaking generally, and we are indebted to Mr. Kirk for the observation. *Colobanthus* (Caryophyllaceæ) is one of those densely tufted moss-like genera of which there are representatives in

various natural orders. It is one of the very few genera common to Australasia, to the Antarctic, and other southern islands, and the Andes, and confined to these regions. One species, *C. quitensis*, ranges from the mountains of Mexico to Cape Horn and reappears in New Zealand. Kirk also records it from Amsterdam Island, but that seems to involve two errors, for, so far as our data at Kew go, *C. diffusus* inhabits St. Paul, and no species is found in the neighbouring island of Amsterdam. One species, *C. Billardieri*, is found in the Alps of Victoria, in Tasmania, New Zealand, and the small islands southward to Macquarie. Two Falkland Islands species also recur in South Georgia, the southern insular limit of phanerogamic vegetation in the Patagonian region, if we except a grass, *Aira antarctica*, collected by Dr. Eights in the South Shetlands, about 62° S. lat., or 8° south of South Georgia. Kirk enumerates and describes ten species of *Colobanthus* from the New Zealand region, including four proposed new ones.

Gunnera (Haloragidaceæ) has a similar range to that of *Colobanthus*, save that it does not reach the colder limits either in America or the New Zealand region. Kirk brings up the species of the latter region to nine, four of which are new.

W. Colenso, D. Petrie, and H. C. Field also describe a few novelties (19), and the first named gives a charming description of his travels and botanising in the romantic country around Hawke's Bay, upwards of fifty years ago.

The one paper which I propose to discuss a little more in detail is devoted to the natural history of Macquarie Island (20), the most southerly speck of land in the New Zealand region known to support phanerogamic vegetation. It is in the same latitude (54° S.) as South Georgia in American waters, the flora of which I have described (21), where a list is given of the vascular plants inhabiting the island. They are separated from each other by about 164° of longitude, which in this latitude means, in round numbers, 5875 geographical miles; yet, as previously stated, nine out of thirteen of the vascular plants found in South

Georgia also occur in some of the southern islands in the New Zealand region. Later on I shall have something to say, or rather repeat, in explanation of this fact. It should be noted that these islands are in about the same latitude as York in England; yet the climate is now so severe in South Georgia and other conditions are so unfavourable to vegetation that the flora is perhaps poorer than in the highest northern latitudes yet explored, and entirely wanting the colour characteristic of many northern flowers. For example, such charmingly beautiful plants as *Papaver nudicaule*, *Silene acaulis*, *Saxifraga oppositifolia* and *Epilobium latifolium* are found north of the eightieth parallel; whereas the showiest flowers in South Georgia are those of a very small buttercup, so small indeed that they want finding. The flora of Macquarie Island is, however, not altogether devoid of colour, as witness *Pleurophyllum;* and *Stilbocarpa* is remarkable for its large rhubarb-like leaves.

Macquarie Island is between twenty and twenty-five miles long and five or six miles across in its broadest part. It is generally hilly, though the hills are nowhere above 800 feet. The following is a list of the vascular plants recorded by Mr. Hamilton (20), who visited the island early in 1894. I may mention that I had most of these plants under observation (22), and I do not agree in every instance with his and Mr. Kirk's (23) determinations; but the divergencies are unimportant; and there are several corrections of the names given in previously published lists. *Ranunculus crassipes*, *Cardamine hirsuta*, var. *corymbosa*, *Colobanthus muscoides*, *C. Billardieri*, *Stellaria decipiens*, *Montia fontana*, *Acæna Sanguisorbæ*, *A. adscendens*, *Callitriche antarctica*. *Epilobium nummularifolium*, *E. linnæoides*, *Azorella Selago*, *Stilbocarpa polaris*, *Coprosma repens*, *Cotula plumosa*, *Pleurophyllum Hookerii*, *Uncinia nervosa*, *Luzula crinita*, *Deschampsia Hookeri*, *D. penicillata*, *Poa foliosa*, *P. Hamiltonii*, *Agrostis antarctica*, *Festuca contracta*, *Aspidium aculeatum*, var. *vestitum* *Polypodium australe*, *Lomaria alpina* and *Lycopodium Billardieri*, var. *varium*. The last named one would have hardly expected to find in so high a latitude, where the

only woody plant is the small creeping *Coprosma repens*, because it usually grows on trees. A re-examination of a very small collection of Macquarie Island plants sent by Mr. Fraser of the Sydney Botanic Garden to the late Sir William Hooker, about sixty years ago, has led to the discovery of *Lycopodium Selago*, associated with *Azorella Selago*, a very similar plant in external appearance. In addition to the foregoing there are three colonised vascular plants, namely, *Stellaria media*, *Cerastium triviale* and *Poa annua*; and Mr. Hamilton states that he also collected *Tillæa muscosa* and two sedges, but the specimens were lost. If we except three imperfectly known grasses, which Mr. Kirk has described as new (24), there are no endemic plants in the island. The vascular cryptogams are all widely spread, two of them recurring in the northern hemisphere. Of the flowering plants upwards of half are confined to the New Zealand region, and the rest have a wider range. *Stilbocarpa polaris* (Araliaceæ) and *Pleurophyllum Hookerii* (Compositæ) are the two most remarkable and most conspicuous plants in this meagre flora; the former having large rhubarb-like leaves, and the latter silky, silvery leaves and handsome purple flower-heads in long racemes. *Colobanthus*, *Azorella*, *Acæna* and *Uncinia* are equally characteristic in the South American region.

Quite recently a fresh account of Lord Howe, Pitcairn and Norfolk Islands has appeared (25), but it contains nothing new on the botany of these islands. Special stress is laid on the beauty of the vegetation of Howe Island, where palms and tree ferns abound, and fig-trees of the banyan type attain dimensions hardly exceeded elsewhere. What is known, however, of the botany of this interesting island has appeared in Government Reports and scattered in a variety of publications (26-29) of limited circulation. It is true that Sir F. von Mueller long ago published (30) a bare list of all the plants known to him from the island, but it is incomplete, and supplies no information beyond the names of the plants. This being so, I am preparing a detailed account of the flora of this island with a view to publication elsewhere. I may here give,

however, some particulars gleaned from the publications referred to, though they are mostly anterior to the date (1885) to which I have limited myself generally in these articles, adding a few remarks of my own on the distribution of the plants.

Lord Howe Island is of small extent and peculiar conformation, situated about 300 miles from the coast of New South Wales in 31° 35′ S. lat. It is seven miles long with an average breadth of one mile, and the steep circular flat-topped elevations rise to a height of nearly 3000 feet. Norfolk Island, the nearest land to the north-east, is about 500 miles distant, and New Zealand, to the south-east, somewhat farther off. The island is of volcanic origin, consisting of three basaltic masses connected by coral-sand rock. About 165 species of indigenous flowering plants are known, and forty-eight ferns and lycopods. As already indicated palms form a conspicuous feature in the scenery. There are four species, all endemic, and they have been very much named, though three out of the four are well known under the generic name of *Kentia*. They are *K. Belmoreana*, *K. Canterburyana* and *K. Forsteriana*—names familiar to many persons, as they have long been favourite palms in cultivation on account of their elegance and hardiness. A tall and graceful specimen of *K. Forsteriana* is one of the finest ornaments of the central part of the palm-house at Kew. The fact of there being a good market for the seeds of these insular palms has led to considerable destruction of the trees to obtain them; but I believe the Government of New South Wales has made it a punishable offence to destroy trees on public territory. Beccari (31) has founded the genus *Howea* for them, which, if accepted, is the only endemic one. There are also four indigenous tree ferns, three of which are endemic. But the banyan trees (*Ficus columnaris*) are perhaps the most striking objects in the vegetation. Several appear in the photographs illustrating Wilson's Report, one of which is said to cover an area of three acres! *Moræa Robinsoniana* is an outlying gigantic member of an African genus of Irideæ very closely allied to *Iris* itself. It is known as the

wedding-flower, and there is a fine specimen of it at the south end of the cactus-house at Kew. *Carmichælia exul* (Leguminosæ) is the only species of a considerable genus, with this exception, not known to inhabit any other country than New Zealand. There are other connections with the flora of the latter country, but they are mostly such as extend to Australia as well. *Pimelea longifolia* and the handsome sedge, *Gahnia xanthocarpa*, are apparently exceptions. In round numbers 25 per cent. of the species of flowering plants of Lord Howe Island are endemic, and 62 per cent. are common to Australia, many of these having a wider range. A few are common only to Australia, New Zealand, and Norfolk Island. The shrubby violaceous genus *Hymenanthera* is an example. The gum trees (*Eucalyptus*) of Australia are represented by the endemic *Acicalyptus Fullagari*, a small Fijian genus differing from *Eucalyptus* in having a calyptrate calyx-limb and separate petals. Two other conspicuous trees in the endemic element are *Dracophyllum Fitzgeraldii* (Epacrideæ) and the screw-pine, *Pandanus Forsteri*. The former is a tree, said to be the largest in the order, attaining the height of fifty to sixty feet. It has the foliage and aspect of a monocotyledon rather than of a dicotyledon. One characteristic Australasian type we miss in the Lord Howe Island flora, and that is *Cordyline*.

When reviewing (32) the newer literature relating to the flora of the Galapagos Islands I found little to add to what had been done by Darwin, Hooker and Andersson; merely mentioning the visit of the United States ship *Albatross*, and Dr. G. Baur's theory of the origin of the fauna and flora. Since then an account of Dr. Baur's botanical collections has been published (33), and the substance has also appeared in an English journal (34), and Dr. Baur himself has written (35) and lectured (36) in defence of his theory of the origin of this group of islands. As previously stated, he contends that the evidence points to the present condition being the result of subsidence; that the islands were formerly connected with each other and at a still earlier period with continental America. Although this theory has been derided, I think the biologi-

cal data strongly favour its correctness, and the soundings given in the map accompanying Agassiz's report (37) of the *Albatross* expedition show a relatively shallow area in which the Galapagos Islands are situated, and which extends eastward to the mainland of Veraguas. Probably the separation would be greatly anterior to the segregation of the West Indian Islands.

In the *Botany* of the *Challenger* expedition (38) I attempted a rough classification of islands in relation to the composition of their floras. These are defined as follows: 1, Vegetation comprising a large endemic element including distinct generic types; 2, vegetation comprising a small, chiefly endemic element, the derivation of which is easily traced; and 3, vegetation containing no endemic element. Without due consideration the Galapagos were referred to the first category. Sir Joseph Hooker (39) fully realised the absolute American affinities of the flora; but he analysed and discussed it as a derived one rather than as a remnant. Darwin, through some misinterpretation of the statistics supplied to him, fell into a singular error respecting the generic endemic element in the Galapagos (40). Referring to the Compositæ, he says: "There are twenty-one species, of which twenty are peculiar to this archipelago; these belong to twelve genera, and of these genera no less than ten are confined to the archipelago!" How this error arose it is impossible to say, but as a matter of fact the statement quoted is wrong (and was wrong at the time it was written) in all its details. With regard to assumed endemic genera of Compositæ, *five* were founded on galapageian plants, namely, *Microcoecia* and *Desmocephalum*, since reduced to *Elvira*; *Macræa* to *Lipochæta*; and *Scalesia* and *Lecocarpus* are so near to *Mirasolia* and *Melampodium* respectively that the late Mr. Bentham gave it as his opinion that they might well be reduced. Two genera from these islands belonging to other orders have also been reduced. These are *Galapogoa* = *Coldenia* (Boraginacæ), and *Dictyocalyx* = *Cacabus* (Solanaceæ); and *Pleuropetalum* (Amarantaceæ) has since been found in several localities in Western America. Taking this view of their affinities, there is not

a single genus of flowering plants endemic in the Galapagos; but each island has its distinct species. Briefly put then, the genera are the same in all the islands, and the genera are American; whereas a large proportion of the species are peculiar to each island, though they are not so exclusively confined to single islands as Darwin supposed. On this point he says (41): "Again *Euphorbia*, a mundane or widely distributed genus, has here eight species of which seven are confined to the archipelago, and not one found on any two islands. *Acalypha* and *Borreria*, both mundane genera, have respectively six and seven species, neither of which genera has the same species on two islands, except in the case of one species of *Borreria*." Dr. Baur's recent explorations necessitate a considerable modification of this statement; yet in a sense they confirm and emphasise it. Baur himself deals more particularly with the fauna (36) in illustration of this phenomenon. More than 400 specimens of the lizard genus *Tropidurus* were collected, and in the result he found that "each island possessed only a single species; all the individuals of an island belonged to one species; and nearly every island had its peculiar species or race".

The botanists who worked out Dr. Baur's collections selected *Euphorbia viminea* (33) as an example of a plant exhibiting racial differences in each of the eight islands, where it is now known to occur. The genera *Acalypha* and *Borreria* are cited as other instances. On the other hand, *Euphorbia articulata*, which was collected on four different islands, showed no such tendency.

In a former article in this journal (32) I mentioned the fact that huge branching Cactaceæ form one of the most striking features in the lower zone of the vegetation of the Galapagos, and I have elsewhere (42) given some particulars of what is known, and how little is known of these Cactaceæ; and I may repeat here that specimens of only one species have, so far as I can ascertain, been brought away from the islands. These were brought to this country by Darwin, and published by Henslow (43) under the name of *Opuntia galapageia*. This species is remarkable

in the genus for its very small flowers, which are only about three-quarters of an inch in diameter, and also for the small number of petals; but as the figure was made from dried specimens, it may be inaccurate in some details. In the same place it is mentioned that a species of *Cereus* was common in the island, but was not found in flower.

Darwin himself specially alludes (44) to the prominent feature these Cactaceæ are in the landscape, and likewise to the fact that they grow in the rough lava where there is absolutely no other phanerogamic vegetation. He further points out their importance as food for the gigantic tortoises and land lizards. They are also a source of water during the severe droughts, which often parch the lower zone.

Subsequent travellers have dwelt upon the part the Cactaceæ play in the biology of the island, and Andersson, a botanist who visited the islands in 1852, states (45) that he observed four or five species, but had time neither to prepare specimens nor sketch the plants.

My note on the subject in *Nature* came under Dr. Baur's notice, and he forwarded me two photographs, one representing a fine example of an arboreous *Opuntia* of great size, and the other a view embracing a number of large *Cerei*, together with a transcript of his notes on the subject in a paper (46) which I had not seen. He was struck by the difference in the appearance of the *Opuntiæ* on the different islands, and observed that the large *Opuntia* has a different habit on nearly every island. Thus, on Barrington, Indefatigable and South Albemarle, it develops a very tall stem; on Charles and Hood a relatively short but thicker stem; on Jervis a very short stem, branching from very near the ground, and on Tower Island it forms no stem at all, and appears as a dwarf bush. Dr. Baur attributes these modifications to the varying degrees of humidity, the greatest development occurring in the driest climate. In the lower region of South Albemarle, up to about 500 feet, the *Opuntia* is very common, attaining a large size, the largest being about twenty feet high, with a trunk two feet in diameter. "In old trees the bark looks

very much like that of a pine, and peels off in very thin sheets."

The common *Cereus*, which strongly resembles *C. peruvianus*, attains almost the same dimensions; but this is all we know about it at present, and there is clearly much more botanical work to be done in the Galapagos before the subject is exhausted. It may be of interest to add that no species of cactus inhabits the island of Juan Fernandez, but this may be ascribed to climatic differences. Indeed, so far as is known, none of the other Pacific American islands, at any considerable distance from the coast, support any members of the order, though Malpelo, for example, is barren enough to give them a chance of flourishing.

Another remarkable element in the flora of the Galapagos is the relatively large number of species of the small order Amarantaceæ. About fifteen species are now known to inhabit the islands, and twelve of them are endemic. They belong mainly to the genera *Telanthera*, *Alternanthera*, and *Froelichia*.

Concerning the flora of the Arctic Islands in relation to the adjacent continents, I have to add a few references (47-48) to works of older date than my paper (49), and a few recent ones of unusual interest. Mr. Trevor-Battye's account of the vegetation of Kolguev Island (50) and Colonel Feilden's contributions on the subject (51-52) rank first among these. The former noted ninety-five species of phanerogamia in Kolguev, and his observations on the vegetation are of great value. About a score of the plants recorded by Ruprecht (53) were not found, and Trevor-Battye remarks on the absence of *Saxifraga oppositifolia*, *Mertensia maritima* and *Ledum palustre*. Colonel Feilden's short paper on Spitzbergen plants, as well as his remarks on mild arctic climates, is worthy of attention on account of his experience. The only information I have found (54) respecting the vegetation of Einsamkeit Island is that there is no grass carpet, and it is added that there is a great quantity of drift-wood, sometimes far inland. A new list (55) of Iceland and Faeroe plants does not claim to be anything more than a contribution to local distribution.

There is little new literature relating to the Atlantic Islands, but Sir Joseph Hooker's comparison (56) of the Maroccan and Canarian floras was overlooked by me when reviewing the writings of Dr. Christ. In an article (57) of more recent publication, the latter gives expression to a considerable modification of his views on the affinities of the Canarian flora. He now recognises a much more intimate connection with the old African flora. But I must not reopen the subject here.

One important contribution (58) to the flora of the West Indies has appeared. This part consists of a critical elaboration of the Myrtaceæ, than which there was probably no group of plants more in need of revision. It is somewhat appalling to see such familiar trees as the allspice and clove with a page and half of synonyms each; yet it is very useful, historically, as well as for practical purposes, to have them brought together.

W. Botting Hemsley.

(*To be continued.*)

THE PRESENT POSITION OF THE CELL-THEORY.

(CONCLUSION.)

THUS far I have tried to rehabilitate the cell as a vital unit. I have now to deal with the further question as to the part played by the cell in the composition of the higher animals and plants. In the earlier part of this essay I stated that Mr. Adam Sedgwick denied *in toto* the proposition that "the elementary parts of all tissues are composed of cells". Since writing those words, Mr. Sedgwick's reply to my previously published criticisms has appeared,[1] and I find that I have made a mistake. For he does not deny the proposition, but says: "The assertion that organisms present a constitution which may be described as cellular is not a theory at all; it is—having first agreed as to the meaning and use of the word cell—a statement of fact and no more a theory than is the assertion that sunlight is composed of all the colours of the spectrum". I can only beg Mr. Sedgwick's pardon. I certainly was led to suppose from his earlier writings that he regards the cell as a nonentity, in so far as it may be considered to be the ultimate structural unit of the metazoa, and I recoiled from his suggestion that the essence of development lay in "a multiplication of nuclei and a specialisation of tracts and vacuoles in a continuous mass of vacuolated protoplasm".

Mr. Sedgwick now explains that he objects, not to the statement that tissues are composed of cells—or, in his own words, that they have a composition which may be described as cellular—but to the statement that an individual metazoon is an aggregate of lesser individuals, or, as it has often been expressed, a cell colony or cell republic. I have elsewhere—and as Mr. Sedgwick well says, after great effort—come to agree with him on this point, for a careful survey of a considerable range of facts led me to the conviction

[1] Adam Sedgwick, "Further Remarks on the Cell-Theory, with a Reply to Mr. Bourne," *Quart. Jour. Micr. Sci.*, vol. xxxviii., p. 331, 1895.

that the idea of a cell republic was inappropriate. Such being the case I would willingly have buried the hatchet, but when I had already dug the hole to bury it in, my hand was stayed by some criticisms on his views and on mine which have just been published in a contemporary periodical.[1] These criticisms have restored to me the conviction which I held when I ventured to write a criticism of Mr. Sedgwick's views; a conviction that, as he originally expressed them, they were calculated to mislead and to do harm to the very cause whose interests he was desirous to promote. As he has lately explained that he did not mean what I supposed him to mean, there is no need for quarrelling any further with him, but he will himself allow that I was amply justified when I gave the following as a not unfair statement of his position. That from the connection known to exist between some cells composing adult tissues, there is an antecedent probability that similar connections exist between all cells composing all tissues; and this probability is heightened by observations made on the development of Peripatus, by the fact that the so-called mesenchyme cells in Avian and Selachian embryoes are continuous and not isolated as was once supposed, and by a study of the developing nerves of Elasmobranchs. And that it follows from this that the morphological concept of a cell so far from being of primary is altogether of secondary importance, and that progress in the knowledge of structure is impossible so long as men persistently regard cells as the fundamental structural units on which the phenomena manifested by organised beings depend. The true method of inquiry must be a study of the growth, extension, vacuolation and specialisation of the living substance protoplasm.

He has been understood by others as I understood him, and indeed he had so expressed himself that he could scarcely have been understood otherwise. What I had anticipated has happened. Persons, ready to grasp at novel ideas, have said in their hearts: "Tush, there is no cell! There are protoplasmic masses which may contain one or many nuclei; the mass is of no importance, it is scarcely

[1] *Natural Science*, vol. vii., No. 46, December, 1895.

more than the medium in which the nucleus lives, and through which it exhibits its powers. The nucleus may move about in the mass, acquiring 'spheres of influence' at its halting places, and so producing the vital phenomena. It is the nucleus which is the vital unit, and there is no bond between nucleus and cytoplasm which shall compel us to regard their union as the necessary condition of living individuality."

I have made use of my own expressions, but if this is not the plain meaning of the short editorial entitled "The Reign of the Nucleus" in the January number of *Natural Science*, what is?

The writer of the editorial is so captivated with the prospect opened up by his interpretation—a perfectly legitimate interpretation—of Mr. Sedgwick's writings, that he forthwith abolishes the existence of cells altogether and talks glibly of "protoplasmic masses," ignoring the fact that the masses in question are divided up into corpuscles. Following up his theme of protoplasmic masses dominated by nuclei, he lightly dismisses the arguments which I put forward, saying that the segmentations of Nereis, Unio, etc., exhibit nuclear lineage rather than cell lineage (who could hold such an opinion after a careful study of Wilson and Lillie's figures?), and winds up with the following astonishing piece of criticism: "In drawing an argument for the cell-theory from the definite places assigned to cells in development Bourne seems to us to have overlooked the experiments of Wilson, Driesch and Hertwig, who have shown that the nuclei may be moved about in the protoplasmic mass almost as freely as a 'heap of billiard balls may roll over each other'". I rubbed my eyes and wondered. I thought I knew the works of Driesch, Hertwig and Wilson pretty well, and that I had considered them carefully, and I had certainly regarded them as strong evidence in favour of the cell-theory as I conceived of it. A short search soon hit upon the passages which are professedly quoted. First for Driesch:[1] "Die Furchungs-

[1] Hans Driesch, *Entwicklungmechanischse Studien*, iv., *Zeitschrift für Wiss. Zoologie*, vol. lv., 1893.

kugeln der Echiniden als ein gleichartiges Material anzusehen sind, welches Man in beliebiger Weise, wie einen Haufen Kugeln durch einander werfen kann, ohne dass seine normale Entwicklungsfahigkeit darunter im Mindesten leidet". (The segmentation spheres of Echinids are to be regarded as a homogeneous material which one may roll amongst one another at will like a heap of balls, without thereby destroying in the least their capacity for development.) No hint whatever of rolling the nuclei through the protoplasmic mass. The statement is made of *Furchungskugeln*, that is of cells, and it is the cells that one may roll about like balls. Not a bad argument for my contention, that the blastomeres of many developing ova are disjunct. If there were any doubt as to Driesch's words a study of figures 39-68 which illustrate his paper would satisfy the most exacting. The blastomeres are unusually distinct from one another, especially in the embryoes illustrated by figs. 63 and 67. Now for Hertwig:[1] "Bei den verschiedenen Modificationen des Furchungsplasma werden die aus dem ersten Furchungskern durch aufeinanderfolgende Theilungen erzeugten Kerngenerationen Theilen des Dotters, die in Eiraum eine sehr verschiedene Lage einnehmen, zugetheilt und mit ihnem zu einem zellkörper verbunden. Die Kerne werden in Eiraum wie ein Haufen von Kugeln durch einander gewürfelt." This is a very complicated German sentence and might well lead to a misunderstanding, but it comes out all right in plain English. "In the various modifications of the divisional processes the nuclear generations, which are produced by successive divisions from the segmentation nucleus, are assigned to a portion of the yolk which occupies very different positions within the limits of the egg, and are bound with it to form a cell body. The nuclei are rolled one over another within the limits of the egg like a heap of balls." This passage is a summary of preceding statements and inferences, and it might be held to bear a very different meaning to that which it does bear; the illustration

[1] O. Hertwig, "Ueber den Werth der ersten Furchungszellen für die Organbildung der Embryo," *Arch. für Mikr. Anat.*, vol. xlii., p. 662, 1893.

of the heap of balls is a very loose one. To understand the meaning of the summary one must turn to pp. 678-685 of the same memoir, which consist of a section entitled "Erklarung des abnormen Furchungsverlaufes". There we learn, as we had previously learnt from Driesch, that the divisional planes of segmenting ova are determined by the direction of the nuclear spindles and that the orientation of the first nuclear spindle is determined by the character of the body of the ovum and its contents. The ova of Echinus are homogeneous throughout, and orientation of the first nuclear spindle is a chance affair. But the ovum of the Frog is not homogeneous; it consists of a smaller cap of protoplasm resting on a large body of yolk, and the nucleus lying in the cap of protoplasm, the direction of the first nuclear spindle is determined by its relations to the more active yolk on the one hand, and the denser food yolk on the other. The relations of the food yolk and protoplasm are changed by the pressure applied during the experiments and the changes are different according as the pressure is applied vertically or horizontally. Hence the direction of the first and the succeeding nuclear spindles is changed in different senses, according to the pressure employed. As the divisional planes are always at right angles to the nuclear spindles, the positions of the two first and the succeeding blastomeres differ according as the pressure applied is vertical, horizontal, oblique, or circumferential. One may in fact cause the blastomeres and their contained nuclei to take up what position one will by varying the direction of the pressure. In this sense, and in this sense only, can one speak of rolling the nuclei about like balls. Not a word about a protoplasmic mass through which the nuclei are caused to roll. On the contrary, a great deal about planes of division and splitting up of the egg into corpuscles round the nuclei. It only requires a glance at Hertwig's figures and diagrams to show that the blastomeres are as distinct during abnormal division as during normal division, and that there is not at any time any question of a "protoplasmic mass," a circumstance which has been well understood by everybody who has taken the trouble to read his memoir carefully.

Most of the experiments of Wilson, Hertwig and Driesch were of a different kind. They isolated the blastomeres by gentle shaking. Driesch is very careful to say *gentle;* rough shaking destroyed the individual blastomeres. Things which are so loosely united as to be separated thus easily from one another scarcely suggest the nature of a coherent protoplasmic mass.

The criticism falls entirely to the ground and one can only wonder how any one could have had the temerity to make it. The very objections urged to my views are but additional evidence in support of them, and I was well aware that the evidence existed when I wrote, but I had to be as brief as possible, and did not refer to it. My statement that it is very clearly established that there are numerous cases in which there is not "a primitive continuity which has never been broken" is abundantly justified. Mr. Sedgwick wonders why I emphasised the distinction and complete isolation of the cells formed by the segmentation of the egg. The reason is surely clear enough. Because he suggested, in no uncertain manner in his earlier writings, that the connections between adult cells were due to a primitive continuity which had never been broken, and that those who urged that such connections were secondary were in the wrong. This suggestion was contrary to fact, and it was my object to show that it was. I did not contradict myself when I stated immediately afterwards that the organism cannot be considered to consist of independent life units, for I went on to show that the cell-republic theory is also contrary to fact, and must therefore be condemned. If a contradiction exists, it exists in nature, and after we have ascertained the facts the next thing is to try to explain this seeming contradiction. Mr. Sedgwick says that he does not think it possible to do so, until we acquire some more understanding of the relative functions of nuclei and protoplasm. Possibly he is right, yet I think that an attempt may be made, and if the explanation is after all not very satisfactory yet some service may be done, for we may arrive at more distinct ideas about fundamental points, and we must gain much by a careful

classification of the facts. Such a classification has yet to be made. So long as a theory is dominant, as the cell-republic theory was, exceptions and difficulties are glossed over, or are explained away by a phrase. When I made a vigorous onslaught on Mr. Sedgwick, I was afraid that he wished to substitute King Stork for King Log and bring us under the domination of a new theory of his own. His reply to my strictures and his careful exposition of his own standpoint are reassuring on this point, and if I exceeded the limits of courtesy in my article, I did so under a misunderstanding and express my regret for it. Mr. Sedgwick has done a great service in breaking the bonds of the old theory. Now the question is, having got our liberty, what are we going to do with it?

Firstly, I think, we have got to make up our minds as to what we mean by a vital unit.

In the first part of this essay I stated that the cell is *par excellence* the vital unit, by which I meant nothing more than that it is the simplest form of material aggregate in which individual life is possible. There would seem to be no objection to such an application of the word unit. But the term unit is a relative one, and its correlative is multiple. If, therefore, we see that the developing embryoes of many animals and likewise the tissues of the adult forms are made up of structures which we must call cells, and if we call the cell a vital unit, we are obliged to conclude that the animals in question are composed of an aggregate of vital units, which leads us directly to the doctrine of a cell-republic. Thus at the outset we are confronted by the great difficulty that what experience teaches us to deny reason compels us to affirm.

There must be a flaw somewhere, either in the facts or in the reasoning. There can hardly be any doubt about the facts; the flaw therefore must be in the reasoning, and I do not doubt that it consists in our insistence on applying the idea of a unit to biological facts. As Whewell would have said, the idea is inappropriate. The term unit, as we use it in Biology, conveys a double meaning. On the one hand, it borrows part of its meaning from the idea of num-

ber, and to this extent the term is used in an equivalent sense to that in which it is used in Physics. But put side by side such expressions as unit of mass or unit of time with the expression unit of life, and a little reflection will suffice to show that the sense is inappropriate. Nor is the case made better if we compare the unit of life with the chemical unit. The value of the latter consists essentially in this, that it is a means of dealing numerically with chemical facts, and experience shows that ideas of number are very appropriate to chemical facts. With life the case is very different. In the present state of our knowledge the connection between life and number is of the slenderest kind, and it is insufficient to justify our applying numerical ideas to vital phenomena.

The other sense in which the term unit is used in Biology is purely subjective. It stands to express our idea of individuality, an idea which is founded on our own states of consciousness. It is unnecessary for me to dilate upon the controversies which have raged round this idea of individuality in its application to the animal kingdom. The most acute reasoners are not agreed upon the precise point where individuality ceases to belong to parts and belongs to the whole even in some of the simpler colonial organisms, and in such cases as the Siphonophora a satisfactory solution of the problem appears to be hopeless.

But these cases are simple in comparison with that which we are now discussing. If then we cannot agree about the limit of individuality in colonial organisms, how are we likely to agree about the same thing in the case of organic structure in general?

There is this to be said, however, that for us the test of individuality should be a biological test, and the idea is therefore more appropriate to the question than the numerical idea just spoken of. It was, no doubt, the recognition of its propriety which lent such force to Schwann's argument, "since it may be proved that some cells, which do not differ from the rest in their mode of growth, are developed independently, we must ascribe to all cells an independent vitality".

Hence, as it seems to me, whilst we can and ought to get rid of the numerical idea expressed by the word unit, we cannot get altogether rid of the idea of individuality, and we must do our best to bring it into harmony with the facts.

Since there is an inseparable connection between the idea of number and the word unit, we ought to get rid of the expression "unit of life," and use some other term which shall denote alike the simplest and the most complex of living beings. The word organism I have already objected to because of its double connotation—would it not be better to make use of such a word as "biont," which is as nearly as possible the equivalent of the German "Lebendiges"? Anything which leads or is capable of leading an independent individual life is a biont. Thus a cell may be a biont, as in the case of the protozoa, or it may be a constituent part of a biont, as in the case of the metazoa. In any case the cell is the simplest form of biont known, for if we go behind the cell we have structures which are not capable of leading an independent individual life.

But a cell in the case of metazoa, or the nucleus and other structures in the case of protozoa, and unicellular plants are things which, whilst they participate in, and contribute to life, and to that extent may be considered as living, are not in themselves capable of independent individual existence. They may be called metabionts.

The terminology suggested may not be perfect, but by the use of it or of something equivalent we may shake ourselves free of the false ideas which have clustered about individual life units, and start with a new hope on an inquiry into the nature and growth of bionts.

An essential part of our conception of a biont is the union of two substances, cytoplasm and nuclein. It does not matter, for present purposes, that we know nothing exact about these two substances, and still less of the manner in which they operate together to produce the phenomena of life. It suffices that we know that there are bionts whose structure is so simple that we can affirm nothing more of them than that they consist of cytoplasm and nuclein, *e.g.*, Bacteria, Yeast, Oscillaria, etc.

Within the limits of the protozoa we study many kinds of bionts which, whilst retaining great simplicity of structure, have advanced far beyond the stage represented by these simple forms.

The most important as well as the most striking structural advance is the formation of a nucleus. The nuclein which was, in the simplest bionts, distributed through the protoplasm, is aggregated to form a compact body, which from its structure and behaviour may be regarded as a metabiont, as also may the part from which it was segregated, the cytoplasm. The steps which lead up to the segregation of the nucleus are obscure, but there are very good grounds for saying that the nucleus, when formed, is connected, in some manner unknown to us, with the transmission of the so-called historic qualities of the biont. In any case it plays a leading part in reproduction, and the steps from the condition of diffused nuclein to centralised nuclein are suggested by the infusorian *Holosticha scutellum*, which ordinarily has no definite nucleus, but contains numerous chromatin particles scattered throughout its substance. Previous to reproduction by division the scattered particles are drawn together and unite to form a centralised nucleus, which divides in a normal manner and breaks up again into particles in the offspring.[1]

Besides the nucleus many other structural advances are to be noted in protozoa and in unicellular plants; some must be regarded as metabionts, *e.g.*, chlorophyll corpuscles and chromatophores of various kinds, many kinds of granules, etc. Other structures cannot be regarded as belonging to the same category, *e.g.*, cilia, contractile fibres, etc. We may for the present purpose leave both cases out of consideration, for it is the nucleus and the part it plays as an essential constituent of the biont which most concerns us.

We have as yet very obscure notions about the co-operation of nucleus and cytoplasm in the production of vital phenomena. But, putting aside the views of those who postulate the existence of minute vital units, and speak of

[1] Aug. Gruber, "Ueber vielkernige Protozoa," *Biol. Centralblatt*, iv., p. 170. See also the same author, *Zeit. für Wiss. Zool.*, xli., p. 186.

an emanation of specialised biophors from the nucleus into the cytoplasm, there is a general agreement that the co-operation is of the nature of a complex exchange of chemical material. If this be the case, the rate of exchange must be the measure of vital activity, and it is clear that the rate of exchange will be greatest in immediate proximity to the nucleus and will become increasingly less the greater the distance from the nucleus. At a certain distance, which might be called the limit of nuclear influence, the rate of exchange will be reduced to zero. We see that in the protozoa the forms which have a single nucleus are small, and we may say, in consequence of the foregoing considerations, that their size is determined by the limits of nuclear influence. But many protozoa are multinuclear, and I believe that there is no exception to the rule that protozoa of relatively large size are also multinuclear. Such is obviously the case in such forms as Radiolaria, Actinosphærium, Pelomyxa, the Myxomycetes and others. From a consideration of all the facts of the case we may legitimately infer that in any given biont growth beyond certain limits is incompatible with a uninuclear condition, and that further growth involves multiplication of the nucleus, which may have as consequences: (1) discontinuous growth, which in its simplest form is reproduction by binary fission: (2) continuous growth, in which the nucleus is multiplied so that all parts of the enlarged cytoplasm may receive an equal share of nuclear influence. There are numerous cases in which, as I pointed out before, the two conditions are combined. There is a cœnocytial[1] stage of considerable duration, followed by reproduction (or discontinuous growth).

The next phase is the formation of a biont of considerable size, in which very numerous nuclei are arranged in definite manner in a continuous mass of protoplasm. Such a condition is represented by the Cœloblastæ, and also in the

[1] When in my earlier essay I coined the word hypopolycytial I was not aware that Professor Vines had applied the term cœnocytial to the Cœloblastæ. His term has the priority and is more euphonious, so I adopt it instead of my own.

growing tissues of many animals and plants, as for instance in the embryoes of many Arthropods, in the endosperm of Phanerogams, etc. The condition may be permanent, as in the case of the Cœloblastæ, or non-permanent, as in the other cases. But in both instances there is a difference from the cœnocytial condition observed in Protozoa, namely, that the multiplication of the nucleus does not lead to reproduction in the form of the splitting up of the biont into as many new bionts as there are nuclei.

In a cœnocytial biont of appreciable size the relations of the various parts to external conditions will tend to become different, and differences of chemical constitution will be set up in the different regions exposed to different conditions. We can see that this is the case in Botrydium, in which root and shoot are plainly marked off from one another, and better still in Caulerpa, in Codium, and in many of the Moulds. Differences in chemical constitution thus induced will mean difference in exchange between nucleus and cytoplasm, and we may infer that, in accordance with these differences, the cytoplasm within the limit of influence of any one nucleus will in time assume a constitution so different from that of the adjacent cytoplasm as to become sharply marked off from it. It will then acquire its own surface tension—the first step towards a cell wall—and will be a separate corpuscle containing a nucleus, in fact a cell. Such a cell however has not come into being as an individual unit joined to its like, either phylogenetically or ontogenetically, but it has from the first formed a part of an organic whole, of which it is nothing more than a specialised component part.

One looks naturally for evidence of this mode of formation of cellular structure in developing Metazoa. The best evidence is to be found, I think, in the segmentation and formation of the layers in many Cœlenterata. In some Cœlenterata—for example, in Renilla—the nucleus divides without accompanying division of the cytoplasm until eight or sixteen nuclei are present, and then the cytoplasm divides and eight or sixteen cells are formed. But of more importance than this is the formation of the layers. From

the considerations stated above we should expect that the changes in chemical composition of the cytoplasm and the correlated changes in the nucleus, in other words the differentiation, would first become manifest in the peripheral parts of the growing cœnocyte, and that we should have a stage in which there was a cellular external layer and a cœnocytial internal mass. We find that in fact in the embryoes of many Cœlenterates the outer layer is divided up early into sharply defined cells at an early period, whilst the central cells retain the character of a cœnocytium; at most the cell outlines of the internal mass are confused and indistinct.

We see also that in the growing tissues of the embryoes of higher animals the embryonic tissue is not cellular but is a cœnocytium, for example, the mesoblast of Avian and Selachian embryoes and of the Rabbit. It is only at a later stage when different relations to other parts of the body have been acquired and new exchanges of material are forced upon the growing mass, that the continuous mass of cytoplasm is split up into corpuscles, each of which, in my view, corresponds to the limit of influence of a nucleus.

On the other hand we have the undoubted fact that in many organisms there is no cœnocytial phase in development, but the cytoplasm surrounding the nuclei as they are successively formed is immediately marked off into definite corpuscles, so that the whole process of development suggests the formation of an aggregate of bionts derived by division from a single parental biont. An explanation of this fact presents many difficulties, and I have not now the space to discuss these difficulties and to show that, obscure as the subject still is, there is ground for supposing that the difficulties are chiefly due to the prepossession which exists in most minds in favour of the independent life unit theory. I hinted in my previous paper (*loc. cit.*, p. 171) that the discrete condition of the blastomeres of so many embryoes may be connected with the fact that they are, from the very outset, specialised. This means that as the nucleus is in some way associated with the transmission of historic qualities, these qualities may be located in special

parts of the nucleus, and on division, some of the daughter nuclei may possess one set, others may possess another set of "qualities". By "qualities" I conceive that we mean different chemical constitutions, and it would follow that the daughter nuclei, being of diverse chemical constitutions, would react in diverse manners on the adjacent protoplasm and would each cause the delimitation of a territory of cytoplasm within the limits of its own sphere of influence; in other words, cell bodies would be formed round nuclei of different chemical constitutions.

There is, however, yet another consideration to be taken into account. As Hertwig has shown, the cytoplasm in many ova is not homogeneous but is obviously separable into tracts of unquestionably different chemical constitution. This is conspicuously evident in the ova of Amphibia. As the nucleus divides, its products come into relation with different kinds of cytoplasm and the exchanges between nucleus and cytoplasm will be different in different places within the limits of the egg. Arguing on the same principles as before, we may attribute the successive formation of discrete blastomeres to this factor as much as to the separation in the course of division of different qualities contained in the egg nucleus, and according as one leans towards an epigenetic or an evolutionary theory of development so will one be disposed to lay more stress on the one factor or the other. There is this much to be said, that the most remarkable cell-lineages (which are only traceable when the blastomeres are discrete) have been observed in ova which contain a considerable proportion of yolk, which is not evenly distributed throughout the egg, and it is suggestive that segmentation in all cases leads to the segregation of corpuscles richer in yolk from corpuscles poorer in yolk—in fact to the segregation of materials of diverse chemical constitution.

Tempting as it is to pursue this subject further, I must not attempt to do it now. But as I have claimed that the views which I have tentatively put forward are agreeable to the facts which we are in possession of, I may well give a short summary of the facts which I have relied upon.

(1) The co-existence of two substances at least, nuclein and cytoplasm, is requisite for life. (This is an inference, strictly speaking, and not a fact; but I think that it may be considered a legitimate inference from what we know of the structure of the lowest bionts, and from the experiments of Nussbaum, Gruber, Verworn and others.)

(2) The existence of bionts, such as Bacteria, in which we are unable to distinguish more than these two substances. (This is a fact, which lends material support to the above inference.)

(3) The existence of bionts in which nuclein and cytoplasm are not indefinitely intermingled, but the former is segregated in the form of particles scattered through the protoplasm, *e.g.*, *Trachelocerca phœnicopterus* and *Chœnia teres*. (We gather from this fact that the two chemical substances tend to become separated from one another.)

(4) The temporary aggregation of nuclein particles to form a centralised nucleus for the purpose of the reproductive act, *e.g.*, *Holosticha scutellum*. (We infer from this that there is some connection, at present hidden from us, between the nucleus and the reproductive act.)

(5) The existence of many bionts in which the nuclein is concentrated to form a nucleus. (We infer that this is a grade of permanent differentiation arising out of the previous temporary grade.)

(6) The existence of many nuclei in all bionts which, whilst still undivided as regards their cytoplasm, attain to a certain size. (From this we infer that the "limit of nuclear influence" cannot extend through a large mass of cytoplasm.)

(7) The origin of "cellular" tissues from a cœnocytial mass, *e.g.*, the endosperm of Phanerogams; the neural crest of certain Vertebrate embryoes; the embryoes of Arthropods; the mesoblast of many Vertebrates, etc. (From this we infer that the cells composing many tissues of higher animals are not to be regarded as bionts, but are secondarily derived during the growth and extension of the parts of a single biont.)

This *résumé* suffices I think to show that this at least

may be claimed for the views which I have put forward. They are founded strictly on the facts, and they do not depend on the assumption of any kind of hypothetical units of which the nature and even the very existence is entirely beyond our ken.

Since I have not been able to develop my views, I cannot but expect that they will be subject to considerable modification and even to entire overthrow. They form at least an attempt to classify and colligate the various phenomena which seem to be germane to the subject, and I have collected and compared a much larger body of facts than I am here able to refer to, without finding any which are contradictory to my ideas. That my ideas are somewhat indistinct need not, at present, be urged as an objection, for indistinctness is not necessarily a sign of falsity. The cell-republic theory was not wanting in distinctness, but it was inappropriate to the facts. I only claim that my ideas are appropriate, and I shall hope to give them more distinctness on a future occasion.

In the meantime I leave out of consideration a large question, concerning which I think it scarcely possible to give a satisfactory account, in this standing in opposition to Mr. Sedgwick, who thinks that which I have attempted to be impossible, but offers a solution of that which I think scarcely possible.

The question is, how are we to account for that phenomenon which I have described as a progress from the state of an independent corpuscle, through a state of many coherent or continuous or conjunct interdependent corpuscles, back again to the state of a single independent corpuscle?

Mr. Sedgwick's solution is this: that the unicellular form is assumed by metazoa in order that conjugation may be possible. The single independent corpuscle which recurs in the cycle is the sexual cell, and the essential feature of sexual reproduction is the conjugation of reproductive cells. The unicellular phase is only assumed in sexual, not in asexual reproduction, and this is to be explained by the consideration that conjugation is as necessary in metazoan life as in protozoan life, but that conjugation between the

ordinary forms of metazoa is impossible for mechanical reasons, and therefore special individuals of a form simple enough to admit of conjugation are produced. These special individuals are the ovum and spermatozoon.

The explanation is extremely ingenious and there is nothing unreasonable in it, but one cannot say that it is altogether acceptable at first sight. It would have been more satisfying if Mr. Sedgwick had marshalled some of the facts relative to the sexual reproduction of some of the lowest multicellular organisms and had shown their relation to his suggestion. A difficulty which at once occurs to me is that in many plants asexual reproduction is effected through the agency of a single cell. In fact, before one can accept any solution of the question one requires a very extensive and careful survey of all the facts known about the reproduction of the lower plants. They afford examples of every conceivable grade of the reproductive processes, and, once one begins to look into the subject, hints as to the parting of the ways of sexual and asexual reproduction occur to one at every step. The pity is that the mere zoologist, who does not find such a fruitful field in his own territory, is obliged to disinter the facts from the load which the peculiarities of botanical terminology have heaped upon them.

It is quite possible, however, that such a survey would afford strong support to Mr. Sedgwick's opinions, and if it should do so they would in no way be inconsistent with the ideas which I have put forward, but would rather support them.

A word in conclusion for those who will reproach me for having attempted to frame a chemico-physical theory of organic growth, and for having used such phrases as "complex chemical constitution," "exchange of chemical material," etc., without assigning any distinct meaning to them. I admit that our knowledge on the subject is rather inadequate, and that I have used obscure phrases to express relations which are in themselves obscure. If one attempts to lift the veil of obscurity one must inevitably

call hypothesis to aid, and it has been my object to avoid the use of hypothesis where I could do without it. It is, however, legitimate to frame an argument which, while it agrees with the lessons of experience, is ultimately based upon hypothetical considerations, provided always that those considerations are consistent with the accepted teaching of the sciences whose aid is invoked.

Any attempt whatever to find an explanation of vital phenomena ends in an appeal to chemistry and physics. Knowing as we do that the elements of which organic bodies are composed are not different from those which occur in the inorganic world, we cannot refuse to acknowledge that vital processes are in the end chemico-physical processes, and this much is conceded by every author of a theory of vital units. The difficulty which they have to face is the same as that which I have to face, and is not one whit the less because it is compressed into the limits of a biophor, whereas I would allow it the limits of a cell. Can we frame any distinct ideas of these chemico-physical processes? Not very distinct ideas, perhaps, yet we can supplement the lack of positive evidence by analogies and illustrations involving the same ideas as those which are current in the physical world.

It was Professor W. K. Clifford, I think, who first drew a graphic picture of the molecular forces which are at work in any chemical compound, by describing the atoms as linked to one another and dancing a sort of merry-go-round within circumscribed limits. We may carry on the illustration, which, fanciful though it may seem, is supported by physical and mathematical considerations. A biont is a great organised war dance, performed by a whole army corps. The individuals composing each company are the atoms, they are linked to one another by companies and each company dances its own figure. Every company is a molecule, and every company dance is but a part of a larger dance, in which the companies act in relation to one another as the individuals act in the company dance. The larger dances are regimental dances and every regiment is a micella. The regimental dances are but parts of still larger

brigade dances, and the brigade dances are but part of the great dance of the whole army corps, which, taken as a whole, is the biont. The illustration is not quite exact, for each company must not be considered as consisting of like individuals, but of many individuals of all arms, some like and some unlike, linked in such various ways that no two companies are the same, partly because of the proportions of different kinds of individuals composing them, partly because of the way in which those individuals are linked together. Nor must we imagine that individuals are permanently attached to companies, nor yet companies to regiments, but that in the course of the dance individuals are passed from company to company, and companies from regiment to regiment, each conforming temporarily to the particular figure of that part of the dance to which he or it for the time belongs. Further than this the individuals engaged in the whole dance are never long the same: there are bystanders who for a time do not participate in the dance but are caught up one by one, whirled through the figures, passed from company to company, from regiment to regiment and brigade to brigade, and are eventually passed out of the dance again, after having participated in some or all of the figures as the case may be. Every individual in the dance is at some time passed out of the dance, becomes a bystander, and may again be caught up and whirled along in the dance once more.

The illustration is fanciful, if you please, but it is of the same kind as illustrations used to depict the play of molecular forces in the inorganic world. It serves a purpose in that it gives the imagination something to work upon, and it enables one to conceive of the immense complexity which is possible in a chemico-physical process. The army dance which I describe is capable of any number of combinations, a number amply sufficient to satisfy the needs of those who insist so strongly on the marvellous complexity of life. Let anybody imagine an army to be composed of four brigades, each brigade of four regiments, each regiment of ten companies, and each company to contain 100 individuals of the eight kinds, carbon, oxygen, hydrogen,

nitrogen, sulphur, phosphorus, potassium and iron, in varying proportions, and let him work out the possible combinations. I think he will be satisfied with the complexity.

What then of heredity and of the capacity which I have mentioned for acquiring historic qualities?

Believing as I do that the vital processes must in the end be attributed to a particular mode of molecular motion, I believe that it is the form of movement which is transmitted. Returning to my illustration I would say that it is the figure of the whole dance which makes up the species, and that it is the figure—the mode of motion—which is inherited, clearly not the individuals engaged in the dance, except in a very small degree, for they are constantly coming into the dance anew and as constantly being passed out of it. Under certain circumstances there may be an excess of one or more kinds of new individuals pressing into one part of the dance which will affect the figure of the company dance which they crowd into, and this will affect regimental figures and ultimately, in decreasing degrees, the whole army figure. In this way we may picture to ourselves the action of external influences in bringing about variation. But I have given rein enough to my imagination. The picture was introduced partly to show that beneath my obscure phrases there was some distinctness of ideas, partly to emphasise the immense complexity of Nature and to show that even atoms and molecules may be conceived to be so combined together that, in Goethe's words, " sie bewirken so eine unendliche Production auf alle Weise und nach allen Seiten ".

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THE HEREDITARY TRANSMISSION OF MICRO-ORGANISMS.

IT is well known that in the construction of many of the theories of heredity the doctrine of the transmission of acquired characters has obtained considerable prominence. The hypothesis of Lamarck rendered it necessary to assume that structural characters which had arisen from the use or disuse of organs, became an integral part of the individual and reappeared in the descendants, and although the application of this idea became greatly restricted when the principle of natural selection was established, it is only within the last few years that the transmission of acquired characters has been considered as unproven, and the instances put forward in support of this view to be capable of a different explanation. It may be admitted that mutilations and permanent injuries can be included among acquired characters, and the structural and functional modifications of the individual which occur in disease may persist, and therefore also be considered as definite morphological or physiological changes. Mutilations apparently do not pass from parent to offspring, and this has been especially pointed out by Weismann and his followers, since, if heredity is capable of explanation on the hypothesis of the continuity of germ-plasm contained in definite reproductive cells, any change in the structure or modes of activity of the essential body or somatic cells would not be transmitted. An identical line of argument also negatives the belief that diseases can be inherited, and this view was maintained by Weismann in his well-known criticism on the transmission of experimental epilepsy; the symptoms in this hereditary disease he considered might be due to some unknown microbe which found its nutritive medium in the nervous tissues and was transmitted in the reproductive cells. The question whether micro-organisms can actually pass from parent to offspring is one which has been carefully investigated, whereas

the proof that actual morphological changes, such as modifications of histological or molecular structure, can be transmitted has not yet been given. It is conceivable that predispositions may be inherited, and these must result from alterations in the germ-plasm, or a direct infection of the germ or embryo might cause the transference of a disease from one generation to another, a phenomenon which simply depends upon a particular mode of conveyance of a parasite.[1]

In many diseases, and particularly those which are directly caused by micro-organisms, it is a matter of interest to note the wide differences which exist between the conveyance of hereditary characters, and of a specific disease. Armauer Hansen (1) has made this perfectly clear in considering the etiology of leprosy. He has pointed out that true hereditary characters are usually limited to one sex, frequently appear at a particular age, and the phenomenon of atavism is not rare; but in the conveyance of such a disease as tuberculosis or leprosy, none of these conditions are fulfilled. It is a logical deduction from the consideration of these differences that every specific disease which is transmitted cannot be regarded as hereditary, but as an instance of the direct bacterial infection of the germ-cells or embryo. Most writers on cancer and malignant growths have discussed the hereditary transmission of this disease, and if it is allowed that a disposition to cancer may be derived by inheritance, then this condition would depend upon some peculiarity inherent in the nucleus of the germ-cells; but if, on the other hand, malignant disease is caused by a parasite belonging, as some investigators have sought to prove, to the group of protozoa or protophyta, then the transmission of the actual disease will depend upon the passage of a micro-organism which invades the germ or its

[1] "Pour les maladies, vraiement constitutionnelles, c'est la substance héréditaire elle-même qui est viceuse; pour les maladies infectieuses, le vice n'est pas dans la substance elle-même, mais à côté d'elle, et les produits sexuels servent seulement de vehicule à un parasite capable d'engendrer plus tard une maladie générale." Y. Delage, *La Structure du Protoplasma et les Theories sur l'Hérédité*. Paris, 1895.

product, and the whole phenomenon ceases to be one of heredity, for the hereditary transmission of micro-organisms is simply a particular instance of bacterial infection. The inheritance of actual specific disease must therefore always be considered as a problem absolutely distinct from that of heredity and incapable of explanation by any hypothesis of heredity.

Micro-organisms which reach an individual either by inheritance or other modes of conveyance may undoubtedly exhibit a period of latent life extending over many years; but when this condition is succeeded by an active life, to establish the proof of an hereditary transmission is exceedingly difficult or even impossible (11). The early researches into problems of this nature were necessarily made with the help of statistical and clinical methods; but it is the application of experimental methods, which could only be pursued with success as the study of bacteriology developed, that has finally succeeded in removing the subject of the hereditary transmission of specific diseases from the hazy region of speculation. The attitude assumed by Baumgarten and his followers on this question is well known. In the case of tuberculosis it is maintained that individuals are rarely infected with tubercle bacilli after birth, but that the disease in the majority of cases is due to a parasitic infection of the egg-cell or embryo. It is even urged that the bacilli may remain latent in one individual, and only enter upon a phase of activity in the offspring, a view which, if correct, would accord with the opinion of many clinical observers. While destroying the opinion so commonly held that an "inherited tubercular predisposition" exists, Baumgarten's theory of hereditary parasitism makes a still greater demand on the imagination; but that the views of this distinguished pathologist have acted as a stimulus to renewed experimental work on the transmission of micro-organisms is beyond doubt. Recent papers by O. Lubarsch (2) of Rostock and J. Csokor (3) of Vienna give an admirable exposition of the present position of our knowledge on this subject of the transference of bacteria from parent to offspring in man and

the lower animals, and the evidence that bacteria may in this manner gain access to the organism is incontestable.

In inherited specific diseases it is possible to distinguish two forms of infection: first, by a direct bacterial invasion of the essential reproductive cells; secondly, the egg-cell or the embryo may receive micro-organisms from the female, in which case the blood stream is the channel for conveyance, and the whole phenomenon is then one of metastasis comparable in every respect to what obtains when bacteria multiply at a definite area of the body, and thence become distributed by the blood and lymph in distant parts of the organism. Bacterial infection may therefore be either germinative or placental, and in mammals the latter form of transmission is not infrequently observed. The specific bacteria of anthrax, typhoid fever (6), pneumonia and tuberculosis (7) have been isolated from the human fœtus, cultivated, and successfully inoculated upon animals, so that the chain of evidence is complete. The pyogenic cocci such as streptococcus pyogenes (24) and staphylococcus pyogenes aureus have also been demonstrated in fœtal tissues by Fraenkel and Kiderlen, and Auché has shown that in small-pox the placenta may be penetrated by these micro-organisms. In the lower animals not only may the bacteria already mentioned be transmitted, but also those of cholera, glanders and chicken cholera.

In many animals the egg-cell is the largest unit of the organism, and would be capable of containing numberless bacteria; that such an infection does occur was first established by the classical observations of Pasteur (4), which have been confirmed by all subsequent investigators. In pébrine, a disease of silk-worms, definite sporocyst forms (microsporidia or Cornalia's corpuscles) are transmitted from the imago in the egg-cell, and the larva is directly infected in this manner. Blochmann (5) has also described a similar mode of conveyance of bacteria in the ova of *Blatta orientalis*. In a single instance a tubercle bacillus has been seen in the mammalian ovum. The sperm-mother-cells may also be invaded by micro-organisms, but this is rare, and no example of an infected male reproductive cell

exists. That this condition will ever be demonstrated is improbable, since bacteria contrast with parasitic protozoa in infecting the cell and sparing the cell-nucleus, and the essential agent in the process of fertilisation is the nucleus or head of the sperm-cell.

Various observers have attempted a solution of this question of germinative infection by the employment of two different methods. The first of these is that pursued by Maffucci, who directly infected the fertilised eggs of the fowl, and in the second not only were the genital glands and the products of these examined for micro-organisms, but pieces of them were taken from animals suffering with specific diseases and used as material for inoculation.

Even if it is assumed that an ovum actually is a site in which bacilli such as those of tuberculosis exist, it may be objected either that the microbe is dead, or that such a cell is incapable of development. This is the attitude taken by Virchow, who absolutely denies the existence of congenital tuberculosis. Maffucci's experiments, however, contradict this opinion, for this observer has shown that the bacilli of avian tuberculosis develop in an infected embryo, and the chicken succumbs to tuberculosis in twenty days to four and a half months after hatching. If, however, instead of infecting the embryo, bacteria such as those of chicken cholera, or anthrax, or Friedländer's pneumococcus are introduced in the extra-embryonic area, then these organisms may actually enter the embryo through the allantois but do not increase in number provided the embryo remains alive. The pathogenic micro-organisms may therefore be destroyed or attenuated by actively proliferating embryonic tissue cells, or they may become capable of development at a later period of life, in other words, remain latent. Although these experiments were devised to establish the view that a genuine germinative infection may occur, they obviously do nothing of the kind, and it is to the researches of Gärtner that we owe an absolute demonstration that ova may contain pathogenic germs. Gärtner among other animals inoculated canaries with mammalian tubercle bacilli. After a few weeks he removed

nine eggs, washed these in dilute corrosive sublimate, dried them in wool and introduced the contents of each egg into the peritoneal cavity of guinea-pigs. In two cases tuberculosis was set up, the animals dying one and a half months and two and a half months after infection. These experiments, which are absolutely free from objection, conclusively prove that the egg-cell may contain virulent bacteria, and it is easily conceivable that such eggs may develop and the transmission of the parasite take place by direct germinative infection, especially since Maffucci's work shows that such infected eggs are capable of development.

Jani, Westermayer, Spano, Walther, Gärtner, and quite recently Jakh, have microscopically investigated the bacterial contents of the reproductive glands, and also inoculated animals with fragments of these organs. With the exception of Gärtner's researches these experiments have not added greatly to our knowledge of the hereditary transmission of bacteria. All the experiments of Westermayer were negative. In fourteen cases of well-marked general tuberculosis no tubercle bacilli could be recognised, and inoculation experiments were failures. The experiments of Jakh (10) were more fortunate. Five inoculations with pieces of the male reproductive gland and its product, taken from individuals dead of tuberculosis, gave three positive results. If the gland alone was used, the experiments were always negative, and of three inoculations with pieces of the egg-forming gland one was successful. It may be admitted that these experiments do not really throw much light on the subject of germinative infection, but Gärtner's researches are of much greater value. He experimented upon mice, guinea-pigs, rabbits, and canaries, these birds being susceptible to mammalian tubercle bacilli. Having inoculated these animals with bacillus tuberculosis, a careful examination was made of the offspring of such tubercular parents. This method might naturally be expected to give a conclusive answer to the question of hereditary infection, and the following information has been gained from these researches: 1. The sperm rarely contains tubercle bacilli—five in

thirty-two cases. Even if micro-organisms exist they are incapable of infecting the egg. In twenty-two (rabbits) and twenty-one cases (guinea-pigs) where the male reproductive gland was the seat of an acute tubercular process, the offspring were never infected. 2. Neither does the male infect the female by way of the sperm. 3. Infection takes place frequently from the female to the fœtus, and in an overwhelming majority of cases by way of the placenta.

A few considerations may make the importance of Gärtner's work more evident. If bacilli exist, as they occasionally do, in the product of the male gland it is probable that this material, like other parts of the body, contains bacteria only a few days before death, for we know that quite an abnormal number of micro-organisms may invade the whole organism during the last days of life. Tubercle bacilli are immotile and therefore will not easily reach the oviduct or egg, a matter of some importance, since it has been shown that in most cases the ovum is fertilised either high up in the oviduct or even at the time of liberation from the Graafian follicle. Stroganoff (12) has also pointed out that the uterine area is sterile, and the secretion of this is bactericidal, in which it resembles sputum (13) or the mucus of the nasal tract which is almost free from germs (14). Lastly, it is well known that a single male morphological unit is sufficient for fertilisation, and if we assume with Gärtner that 100 virulent tubercle bacilli are mixed with sperm-cells, the ratio of bacteria to these would be about 1 : 22,500,000; it is hardly conceivable on the doctrine of probabilities that a bacillus would gain access to the egg. It may therefore be considered, both on experimental and theoretical grounds, that a germinative infection of the ovum never occurs by the conveyance of micro-organisms in the male reproductive cells.

The difficulties which exist in proving that the inheritance of a specific disease may occur through an infection of the ovum are fortunately not so great in those cases where the passage of micro-organisms takes place solely from the female to the fœtus by way of the placenta. It is established that specific micro-organisms can pass by this

route. It is not even necessary to assume that there is any lesion whatever in the placenta or that the epithelium of the fœtal villi is destroyed. An experiment by Zuntz shows clearly that particulate material will easily pass into the amniotic fluid from the maternal portion of the placenta, for if indigo-carmine is injected into the veins of the female the dye passes into the amnion leaving the fœtus free, and in this very manner anthrax bacilli may pass, and from the amnion gain access to the mouth of the fœtus, enter the gut and set up disease by a primary infection of the wall of the intestine (25). An intra-uterine infection, therefore, can occur without lesion of the placenta, though in the majority of cases this structure is primarily infected, and then the fœtus, or else hæmorrhages of the placenta permit the passage of micro-organisms. However the undoubted fact that micro-organisms can penetrate the healthy skin by way of the hair follicles—and the same is possibly true for the epithelium of the urinary tract—must not be forgotten in considering the passage of bacteria across the placenta. This structure may be normal and even then allow the transit of bacteria. Birch-Hirschfeld (15) from researches on the structure of the human placenta as well as that of mice, rabbits and goats considers that the bacilli of anthrax at any rate can traverse the uninjured chorionic epithelium. Moreover in the human placenta and in rabbits numerous processes of the chorion traverse the placental sinuses, and these processes are normally destitute of epithelium. It was noticed by Max Wolff (16) that anthrax bacilli easily pass if the placenta was crushed or torn, and micro-organisms which exert a necrotic influence on tissues, such as the pyogenic cocci, appear first to destroy the epithelium of the chorionic villi, and then pass through into the fœtal blood. In this fluid micro-organisms reach the liver, and it is this organ which, as a rule, is primarily affected, and then the glands in the lymphatics leading from the organ become implicated. The location, therefore, of tubercles in fœtal tuberculosis is characteristic, and all observers insist upon this feature in determining whether tubercular

deposits are of intra- or extra-uterine origin in early cases of the disease. As a matter of interest it may be mentioned that quite recently Bar and Rénon have demonstrated tubercle bacilli in the blood of the umbilical vein (7). The method used by these observers, that of inoculating guinea-pigs with the suspected blood, and in this manner establishing tuberculosis, is not so convincing as the actual demonstration of bacteria in fœtal tissues. Wassermann (17) in a recent paper especially insists on this point, and discards all evidence of inherited disease which rests simply upon inoculation experiments. He describes a case of early tuberculosis which ended fatally when the child was ten weeks old, where the disease was acquired, not from the parents who were healthy, but by direct infection from a tubercular relation, and believes that such cases as these are not infrequently cited as instances of congenital disease. In his opinion hereditary transmission of bacteria does occur, but it is exceedingly rare in comparison with the frequency of extra-uterine infection. Bernheim (18) considers that the offspring rarely, if ever, become tubercular if separated from tubercular parents, with the exception of those cases where the placenta is infected. The case reported by Ivan Honl (19) of a child fifteen days old that on autopsy showed tubercular nodules in the liver, spleen, and lungs, and numerous bacilli, must be classed as a definite case of transmission which with many others lends no support to Eberth's statement that individuals do not inherit tuberculosis but acquire it (23).

A recent case of congenital typhoid fever is related by Freund and Levy (20), and instances of transmitted hæmorrhagic infection have been recorded by Neumann (21) and by Dungern (22). The numerous examples which the journals of veterinary science contain, especially the work of Bang, Kockel, and Lungwitz, also afford conclusive evidence of the transmission of pathogenic micro-organisms, though there is a consensus of opinion that the placental is far more frequent than the germinative infection. The share borne by the male in this transmission may be disregarded, as no bacteriological evidence exists to support

this view. Finally, the frequency of hereditary transmission of pathogenic germs is exceedingly small compared to other modes of infection.

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